

What Future for Energy?

It is no accident that the United States has slipped from preoccupation with what used to be called an environmental crisis to preoccupation with the now more topical energy crisis. In many significant ways, the restrictions on development which have been adopted in the past few years, on the deployment of nuclear power stations and the exploitation of off-shore petroleum reserves, not to mention the restrictions coming into force on the design of automobile engines, have helped to diminish the supplies of several fuels. In several states, for example, there is already talk of gasoline rationing during the coming year—the oil companies are telling distributors that they will have to be satisfied with as much gasoline as they were able to sell last year. The reasons appear to be more a consequence of a shortage of refining capacity and distribution facilities than of a physical shortage of crude petroleum. The coming year is also likely to see further restrictions in the United States on the use of natural gas, if only because the plan to import this material in liquid form from North Africa—the formal agreement was signed only last week between a consortium of American utility companies and the Algerian Government—will not begin to yield deliveries of natural gas until 1975. Then the shortage of electricity—and the prospect of load-shedding in the summer—is likely to continue at least until the electricity utilities can recover the ground lost in the past few years by the difficulty of finding sites for generating stations, conventional and nuclear. In all the circumstances, it is high time that the United States took a firmer grip on these problems and, in particular, that it should distinguish more clearly between the present “crisis” and the more distant problem of how to keep itself adequately supplied with energy.

The starting point for these discussions is, or should be, clear recognition that it is not possible to transform the character of an industrial economy overnight, by the enactment of legislation. The consumption of energy in the United States is much greater than in other countries, and has for the past few years been increasing as quickly as in any closely comparable economy. Between 1971 and 1972, the total consumption of energy in the United States increased by 4.9 per cent and, because of the inevitable limitation of domestic supplies of hydrocarbon fuels, imports of all kinds of fossil fuels increased by 24.5 per cent in the same period, with the result that imported fossil fuels now account for more than 12 per cent of all the energy consumed in the United States. There is no great surprise in these figures. Indeed, what is happening now in the United States is very similar to what happened in Britain and other European countries in the mid-1950s. Supplying a rapidly growing economy from exclusively domestic fuels is bound to be difficult, not merely because of the problems of maintaining capital investment in fuel extraction processes at a sufficiently high level but also because there are positive advantages in the kind of flexibility which a substantial import programme provides. And the truth is that for the time being, there can be no

marked change in the pace of growth of the demand for energy without a radical and often painful change in the pattern of energy consumption.

How is this to be done? In spite of all the breast-beating about the energy crisis now in vogue, the mechanisms for regulating demand remain much as they were when Adam Smith first outlined the operation of the laws of supply and demand more than a century ago. Energy remains an exceedingly cheap commodity in the United States. The retail price of petrol in Europe is 2.5 times the retail price of gasoline in the United States, even after the recent bout of price increases. Is it unreasonable that there should now be a steady increase of price? Such a development is, for one thing, inevitable as the proportion of imported petroleum increases. Its immediate consequence could be a beneficial economy in the use of gasoline, and there is even a possibility that some of the freight now transported across the United States by road would find its way back to the railways, which are even more acutely in need of the business than they have ever been. The price mechanism has also an important part to play in the regulation of demand for natural gas. For decades, the Federal Government's regulatory agencies have pursued a policy of restricting the prices which can be charged for natural gas to levels at which the utility companies can make a respectable, but not excessive, return on the capital invested in their works. Might it not be better to let prices rise, and to recover at least a part of the extra profit by taxation?

It is also important that the United States Government should make a more hard-headed calculation of the economic cost of some of the environmental measures which have recently been adopted. The regulations now coming into force for the control of exhaust emissions from automobiles are a splendid illustration of how damaging can be over-hasty action in this and similar fields. It now appears—as the more sanguine motor-manufacturers have been predicting, that internal combustion engines designed so as to reduce the output of carbon monoxide and sulphur dioxide to the levels laid down for 1975 will consume an extra 30 or 40 per cent of gasoline. Even the new automobiles manufactured to the 1973 standards are reckoned to need an extra 10 per cent or so of fuel. Given that transportation is responsible for about 60 per cent of petroleum consumption in the United States, this is a splendid way of making sure that the demand for petroleum will increase still more quickly than it has done in recent years. But there are serious doubts about the suitability of the emission standards as a means of ensuring the cleanliness of urban air. The motor manufacturers have a good case when they fear that the catalytic converters with which new cars will be fitted in 1975 will be as effective as they are supposed to be. There are good reasons for believing that the intended result could in any case be achieved more certainly by a sensible regulation of traffic in city streets. Why impose a serious economic penalty on those who transport themselves in



the outlandish parts of the United States, and at the same time exaggerate the shortage of petroleum, when the real problems are confined to the large cities such as Los Angeles and New York? And in any case, it now appears that the most hopeful way of making clean internal combustion engines is by the more radical redesign of the internal combustion engine to make sure that combustion is more complete. In the past few days, the Environmental Protection Agency has been hearing evidence from the motor manufacturers pleading for an extension of the time required for the imposition of the new standards from 1975 to 1976. If it has the courage, it should respond not by a mere extension of the deadline by one year but by an acknowledgement that the timescale now foreseen is altogether too short, and that it would be more prudent to wait until the 1980s for a radical and more satisfactory solution of the problem of how best to design clean internal combustion engines. To fail to do so will not merely make a nonsense of the earnest but over-pious belief that clean air is a part of the modern heritage, but will also seriously hamper the efforts of the United States to strike an economic balance with its trading partners.

Other conflicts between environmental considerations and the need to keep the world's largest industrial economy in being call for equally fierce action. In this sense, it is a serious scandal that the project to carry petroleum from the North Slope of Alaska to the metropolitan United States should have hung fire for the best part of three years, as much because the United States Government has run away from a decision between the conflicting demands of the environmentalists and the power consumers as for any other reason. Yet the simple question is whether the United States will eventually manage without Alaskan petroleum or accept the environmental nuisance which a pipeline would bring. To postpone a decision, apparently in the hope that a solution to the conflict will eventually emerge, is a recipe for making sure that the petroleum, if it ever starts to flow, will arrive too late. But there are other fields in which a more courageous policy by the government could help to ease the immediate shortage of energy. The siting of nuclear power stations is one obvious field where a sense of resolution by the government could help enormously to take the edge of impending problems. So is the strip-mining of coal in the Western states. On all these local issues, it is entirely proper that the Administration should take account more zealously than in the past of the environmental case against thoughtless development, but who else but the Administration can strike a proper balance between the needs of industry and environmental considerations?

By means like this, there is every prospect that the immediate problems of energy scarcity could be brought within control in the measurable future. Fuller use of the price mechanism and more resolute decision-making by the Federal Government are urgently necessary, but could make the immediate "crisis" go away. By the 1990s, however, the United States will have more serious problems to contend with. It now seems clear that the annual import bill for petroleum products is bound to increase steadily, and the chances are that even by 1980, the United States will be spending \$20,000 million a year (or more if there are further devaluations of the dollar) on imported crude petroleum. Numerically, this expenditure is not insupportable, but there are political snags. The Administration is, for example, alarmed at the prospect that so much cash will find its way into the hands

of Arab states which have no automatic sympathy with United States policy in the Middle East — rather the reverse — and which could use their holdings of large quantities of dollars as a way of manipulating the international currency market. But even this is not an entirely unwelcome prospect, for at the very least it will create a climate in which some lasting political settlement in the Middle East will seem to everybody desirable. And here luckily, there is plenty of time in which the United States can prepare the ground for a lasting policy. To be sure in the meantime it will be prudent also to pursue as vigorously as possible the technical means by which other sources of energy might be exploited, and no doubt there is much to be done to make use of geothermal steam and solar energy. But the problem of the 1990s is already definable and is largely political in character. It is to be hoped that the United States will not let that blow up into a crisis for lack of resolution in the next few years.

100 Years Ago



SCIENCE AND THE PRESS IN AMERICA

(FROM A NEW YORK CORRESPONDENT)

THE visit of Prof. Tyndall has given an extraordinary impulse to scientific affairs in this country. It took place at a fortunate moment, just after the heat and turmoil of a presidential election had been transformed into the national sorrow over the death of the defeated candidate; just before the exposures of corruption, which have since disgraced eminent public men, had begun to absorb popular attention. It therefore happened not only that men's minds were not preoccupied, but that, in addition, newspaper columns were not specially crowded. Hence all the leading newspapers gave more space than would have otherwise been possible, to reports of Prof. Tyndall's lectures. In this particular, however, one paper surpassed the rest, giving the lectures verbatim and with illustrations, and afterwards reprinting them in a separate sheet, which, as you are probably already informed, attained a special circulation outside that of the newspaper, of more than 200,000 copies. It is not improbable that this enterprise on the part of the *New York Tribune* originated in a programme for the management of that paper laid down by the late Mr. Greeley. This was printed in its columns the second day after the election, when he resumed his position as editor of the paper. The card specified among other things, first, that thereafter the paper would be enabled to give "a wider and steadier regard to the progress of science, industry, and the useful arts." His successors in the management of the paper have been anxious, for obvious reasons, that it should tread the path he had marked out for it; Tyndall's coming furnished the first opportunity. Other papers have been stimulated by the popularity of scientific topics which the success of these lectures revealed, and there never was a time when such themes found such general acceptance with the newspaper press.

From Nature, 7, 445, April 10, 1873

OLD WORLD

Sir Frederick Dainton to be Chairman of the UGC

SIR FREDERICK DANTON, Dr Lees Professor of Chemistry at the University of Oxford and chairman of the Advisory Board of the Research Councils (ABRC) is to succeed Sir Kenneth Berrill as chairman of the University Grants Committee later this year.

As forecast in last week's *Nature*, Sir Kenneth is to succeed Sir Donald MacDougall as the government's Chief Economic Adviser, but the announcement of Sir Frederick's appointment comes as a complete surprise.

Sir Frederick has played a part in government science for several years and before he was appointed chairman of the ABRC on its inception last year he was chairman of the now defunct Council for Scientific Policy. But Sir Frederick is not cutting himself off entirely from the ABRC for his new position entitles him to a seat on that council. The University of Oxford, however, will be that much poorer for Sir Frederick is resigning his position as Dr Lees Professor of Chemistry.

The decision to leave academic life, said Sir Frederick this week, was one of the hardest that he has had to make. He will miss working with young people and he will miss as well teaching and

research. But Sir Frederick is adamant that he will still keep an active interest in science.

The Advisory Board for the Research Councils, which has now been at work since the autumn, has proved to be more successful than Sir Frederick had at first hoped. It has been a successful forum for the government departments to air their thoughts about research and as a result a great deal of mutual confidence has been built up between the research councils and the departments.

Commenting on the recent government white paper which will change the balance of arts and science students in British universities in the next five years in favour of the arts side, Sir Frederick said that he is concerned that well qualified non-scientists are unable to secure places in universities. In many cases the

qualifications of these art students are better than those of the science students who do manage to get a place. But the white paper will do much to remove this inequity.

Looking to the future and the consequences of a differing rate of growth of the student population and the science budget in the coming years, Sir Frederick said that he is not completely adverse to the concept of university lecturers who do not research. But such lecturers, said Sir Frederick "should breathe the air of a good research department". Sir Frederick also said that it is a bad thing to make a good teacher, who is a reluctant researcher, actually do research. By the same token a good researcher who has no interest in teaching should not be forced to teach.

ECLIPSE

Science with Concorde

THE Science Research Council is to provide £40,000 for the support of experiments to study the total eclipse of the sun which will be observed from North Africa on June 30.

There will be two sets of experiments. The first, consisting of five or six experiments, will be carried out by British, French and United States scientists on board Concorde 001, while the second, which will be a collaborative venture with scientists from the United States and Canada will be on an Aerobee rocket which will be supplied by the Kitt Peak National Observatory in Arizona.

The British experiments aboard the Concorde will be prepared by scientists from the University of Aberdeen and Queen Mary College, University of London. The British contribution to the experiments aboard the Aerobee rocket will come from the Astrophysics Research Division of the Science Research Council's Radio and Space Research Station and Imperial College, University of London.

The Concorde flight will start from Las Palmas in the Canary Islands and will join the path of the eclipse in Mauritania in West Africa at 10.45 a.m. Greenwich Mean Time on June 30. It will follow the eclipse for almost an hour and a half covering 1,900 miles in that time. This will extend the possible observing time to eleven times that which

will be possible for ground based observers. It is planned that the aircraft will fly at 60,000 feet and so will be well above the layers of the atmosphere which absorb infrared radiation. The French contribution to the cost of the flight will be £130,000.

The University of Aberdeen experiment aboard Concorde will be designed to observe, through a side window in the aircraft, radiation from the near infrared region which is emitted from the stratosphere and mesosphere. From these observations, Dr M. Gadsden of Aberdeen and colleagues hope to deduce the decay times of metastable molecular oxygen. Spectra will also be taken from different heights in the atmosphere in order to determine how the decay time varies with height.

The other British experiment aboard Concorde will be operated by Dr J. E. Beckman and colleagues at Queen Mary College, and it will be designed to obtain a spectrum of the chromosphere in the sub-millimeter region. In order to obtain this spectrum a rapid scan Michelson interferometer with a detector cooled by liquid helium will be used.

The British teams will be joined in the Aerobee experiments by teams from York University in Toronto and Harvard College Observatory, while Kitt Peak National Observatory will provide the rocket, launch and recovery operations. The rocket will be launched from a site in Mauritania near Nouadhibou at 10.33 a.m. Greenwich Mean Time on June 30.

Pay System Review

THE government agreed late last week to refer the question of scientists' pay within the Civil Service to an independent review by the recently set up Pay Board. This review will take place in the second part of the year.

The government has also agreed that it will be the long term basis of determining scientists pay that will be under review and this will not be influenced solely by short term economic considerations.

Both the official side and the Institution of Professional Civil Servants will be bound by the Pay Board's decision. The IPCS has been demanding an alternative to the current pay research system for eighteen months, but, until now, the government has failed to agree to an independent review. Under pay research the salaries of scientists working in the Civil Service is decided by comparison with the pay of scientists in industry.

ASTRONOMY

Future of the INT

PROFESSOR GEOFFREY BURBIDGE has, once again, publicized his views on the current state of optical astronomy in Britain. In the correspondence columns of *The Times* on Tuesday of this week, Professor Burbidge, writing from the University of California at San Diego, calls for the Isaac Newton Telescope (INT), now situated at Herstmonceux in Sussex, to be "moved to a good site very quickly". He also calls for an optical observatory to be constructed in the Northern Hemisphere as soon as possible.

It is seven months since Professor Burbidge first gave vent to his feelings about the state of optical astronomy in Britain (see *Nature*, 239, 117; 1972). At that time Professor Burbidge's comments were prompted by the resignation of Professor Sir Fred Hoyle from the Plumian Chair of Astronomy and Experimental Philosophy at Cambridge, and his forthright views made many astronomers respond, first in the pages of *Nature* and then earlier this year in the columns of *The Times* (see *Nature*, 242, 4; 1973).

In spite of the fact that there are 1,900 hours of sunshine a year at south coast resorts near the site of the INT, Professor Burbidge claims that this bears no relation to the time during which the telescope can be used. The INT can only be used, says Professor Burbidge, for 600 to 800 hours a year, whereas the telescopes in California, Arizona and Chile can all be used for up to 2,500 hours a year. But even during the time when the INT can be used, says Professor Burbidge, clouds are frequently present so "that some types of astronomical observations are quite impossible". Very faint objects cannot be observed at all so that cosmological investigations are largely ruled out.

Professor Burbidge, however, is fully aware of the implications of his suggestions that the INT be moved abroad and that a new observatory be built in the Northern Hemisphere. The proposal to move the INT which Burbidge claims has the support of "the majority of the astronomers at the Royal Greenwich Observatory, a large proportion of the younger optical astronomers in the United Kingdom and the expatriate British optical astronomers", is being soft pedalled not because of the cost but because of the fear that if the money is allocated to move the telescope then the government might be reluctant to provide a greater sum of money to build an observatory in the Northern Hemisphere.

A spokesman for the Science Research Council said this week that the council did not have any firm

figures for the costs involved in moving the INT and that it would be some time before a firm decision is made on whether or not to move the telescope.

Hidden in Professor Burbidge's letter to *The Times* is a plea that the decision on whether to build an observatory in the Northern Hemisphere should not be delayed by discussion of "exotic schemes". Professor Burbidge calls for a conventional telescope to be installed in the proposed Northern Hemisphere observatory and says that suggestions that a very large telescope should be built or that an array of telescopes be installed should be ignored. Such schemes might take a long time to build—as well as being expensive—and "there is a very good chance that the British would then have constructed a second astronomical white elephant".

SELECT COMMITTEE

No Need for a PhD

THE better the qualifications of a scientist the less chance he has of being employed in industry and big business. This was the sobering thought which the Confederation of British Industry shared with the Select Committee on Expenditure in Higher Education this week.

The CBI, although realizing the need for graduates trained in research techniques, claims that there is no room for the true specialist in industry and that MScs and PhDs are out of their depth once they leave the ivory towers of universities.

The CBI also complained that universities absorb their best graduates, spending many thousands of pounds producing scientists only interested in pure research. A few of these remain in the university after obtaining a higher degree, expanding the boundaries of knowledge, but the remainder are left to find employment in a highly competitive economic world for which they are not suitably trained.

This, according to Mr Campbell Adamson, Director General of the CBI, is not in the national interest. "We want the graduates with good first degrees . . . if they still want to become doctors they should return to university after a few years in industry." Another complaint of the CBI is that too much money is spent in "producing geniuses who immediately get bought up by other countries".

A possible solution, the select committee was told, is for a university to choose its postgraduate students not only on academic achievement but also on personal qualities which indicate their ability to be tomorrow's "top men". This selection could even be done by an independent selector who is in touch with the needs of industry.

AGRICULTURE

Optimism for All

from a Correspondent

OPTIMISM was the order of the day at a recent Royal Society meeting on agricultural productivity in the 1980s. Over 200 agricultural scientists and several farmers heard the future of food supplies discussed with none of the gloomy forecasts about soil deterioration or ecological doom given the prominence which is usual at conferences that impinge on the environment.

Speakers dealt mainly with the situation in Britain, but Dr A. H. Boerma of the United Nations Food and Agriculture Organization struck a somewhat discordant note early on by drawing attention to the difficulties facing developing countries with high birth rates in even maintaining their *per capita* food production. Overproduction in North America and Europe is unlikely to solve the global problem which, he said, is chiefly caused by political and sociological factors.

Professor J. Ashton, an economist, predicted that farming would become more specialized and capital-intensive, while Dr F. C. Lindvall of the California Institute of Technology foresaw a great increase of mechanization throughout the world and of the "power applied per unit of land". He also thought that the manufacturers would have to pay increasing attention to the prevention of pollution and erosion, but such problems, he said, are not insuperable.

Several speakers stressed the need to preserve the whole environment, while Lord Walston saw a growing need for the government to take action to preserve the environment for the benefit of the nation.

In discussing methods of increasing agricultural efficiency Dr W. F. Edson of Fisons thought that even more use will be made of chemical pesticides, that these would not damage the environment, and that non-chemical methods of pest control would only make a minor contribution. Dr A. B. Paterson from the Central Veterinary Laboratory, Weybridge, forecast that disease prevention would contribute to increased productivity of all livestock.

Dr N. W. Simmonds of the Scottish Plant Breeding Station argued that plant breeders would continue to produce increased quantity and quality in crops, while Professor A. W. Holmes of the British Food Manufacturing Research Association spoke of substitute foods, mainly proteins from soya beans, producing samples aimed at the high-quality market for the audience to try.

But the most thought-provoking part of the meeting was Sir Joseph Hutchinson's final address which, except for Dr

Boerma's contribution, was the only one to descend from the general level of euphoria. Sir Joseph stressed the dangers of a purely economic approach—a surplus of five per cent at present could mean financial ruin for the producers, he said, while a shortage of a similar amount could mean starvation and death in some areas. The need for a global food store is obvious. He also queried the wisdom of introducing capital-intensive farming to developing countries with the result that an unemployed urban proletariat is created. Labour-intensive techniques might be wiser, he said.

Sir Joseph was the only speaker, apart from Dr Boerma, not convinced that all forecasts would be reached without more knowledge arising from increased research on the soil and its potential, which he described as the "patient Cinderella" of agriculture.

ISRAEL

Political Scientist

DR EPHRAIM KATCHALSKY, the noted Israeli biophysicist, is to become President of Israel next week. Dr Katchalsky, who is head of the department of biophysics at the Weizmann Institute, has been nominated by the Israeli Labour Party as its candidate for president. That means certain election as president on April 10 when the Knesset, the Israeli parliament, meets. He will take office in May.

Dr Katchalsky, who was on a lecture tour of the United States when he heard the news, is well known for his work on the physics of large molecules and membranes. He was the first to synthesize polylysine, a molecule that is much used in immunological research.

The post of president in Israel is largely that of a figurehead and has always been filled by an intellectual rather than a politician.

When he takes office Dr Katchalsky will be the fourth president of the country, his predecessors being Dr Chaim Weizmann, founder of the Weizmann Institute, Mr Itzhak Ben-Zvi and Mr Zalman Shazar, who is now retiring, aged 83, after ten years in office.

Dr Katchalsky was born in Kiev in the Soviet Union in 1916 but moved to Israel when he was a child. He was educated at the Hebrew University in Jerusalem and has held various government posts in Israel, experience that will serve him well.

When the news of his appointment came through Dr Katchalsky was in Berkeley in California attending a three day symposium held in memory of his brother Professor Aharon Katzir Katchalsky who was killed in the Lod airport massacre in May 1972 (see *Nature*, **237**, 304; 1972).

COMPUTERS

Wake for Atlas

THE original ICL Atlas I computer at the Science Research Council's Atlas computer laboratory was closed down last week after more than eight years' service. In the closing ceremony Sir Brian Flowers, chairman of the Science Research Council, switched the Atlas off after Professor David Howarth, who wrote the operating system for the computer, had run the last programme.

Atlas's work has now been taken over by an ICL 1906A, which was installed in 1971. This computer is some three times faster than Atlas.

Atlas was designed at the University of Manchester between 1957 and 1961 by a team led by Dr Tom Kilburn. Among the features that made it one of the most advanced machines in the world at the time were a number of facilities that have since become standard such as its permanent master programme and paged store.

The Atlas was ordered in 1961 and installed between 1962 and 1964. For its day it was fast. Simple operations such as addition took about 2 microseconds, and it could solve 100 linear equations in about 10 seconds. Since Atlas was built, however, machines 20 times faster than Atlas have been introduced.

Now that it has been switched off, what is to happen to the computer? The answer is that bits of it will find their way into museums all over the world and the rest will be sold for scrap. Dr J. Howlett, Director of the Atlas Computer Laboratory, said this week that "if some university could install and run it we could probably give it to them" but the problems of moving and maintaining such a large and comparatively ancient machine are enormous. ICL's contract to maintain Atlas expires next year, and finding spares for the machine, if it goes wrong, is likely to be a problem soon. The Atlas Laboratory intends to keep the front end processor with its 100 million character disk, as this is a self-contained unit, and a couple of the Ampex TM2 1 inch tape decks may stay in use although nominally they are obsolete. The Chilton machine is the last but one of the six Atlas computers built to be scrapped. Only the Atlas at the Department of Trade and Industry's computer Art and Design Centre at Cambridge is still running.

During its eight and a half years' operation the Atlas has been available 97 per cent of the time and has handled 836,000 jobs valued at £10.8 million. More than 2,300 university projects have been put onto the computer.

Sir Brian said during the ceremony that the SRC is currently considering

the successor to the ICL 1906A. The problem is one of trying to predict the needs of scientists and then finding a machine that will do the job rather than choosing a machine and then seeing what can be done with it.

TRIBOLOGY

Satellites and Friction

TESTS are to begin this week on solar paddles for the European Space Research Organization's communications satellites.

ESRO's Space Tribology Laboratory (ESTL), which is situated within Britain's National Centre of Tribology at Risley in Lancashire, was founded a year ago and in that time six 40 cm ultra high vacuum chambers have been installed in a clean room that is now fully operational. Six slow speed solar paddle drive mechanisms, capable of one revolution every twenty-four hours were due for delivery a few weeks ago and are to be installed this week. Three each of two designs produced by Hawker-Siddeley and Marconi are to be tested and some of the tests will run for seven years—the minimum life of a communications satellite.

The ESTL contract which is worth £400,000 over four years is the largest feather in the National Centre of Tribology's cap, but the centre has been plugging happily away as a contract research organization for five years during which time, apart from the ESRO contract, it has acquired £0.25 million of business. Contracts have ranged from consultancies costing a few hundred pounds to development contracts worth tens of thousands of pounds and the tribological problems the centre has tackled have ranged from making gas bearings to designing window hinges for high rise flats, from pop-up toasters to pulverising plants and artificial heart valves.

The centre's work falls roughly into three equal parts, the ESRO contract, industrial contract work and work on the Atomic Energy Authority's reactor systems. It was the UKAEA's need for moving parts in the hostile environment of reactors where temperatures are high and service is difficult or impossible that led to the centre's formation. Originally part of the materials group at Risley, the centre began to apply commercially the knowledge gained in developing Magnox and AGR reactors.

Now the contract work plays a larger part in the centre's programme than the UKAEA work, but twenty per cent of the centre's efforts still goes into work on the sodium cooled fast reactor.

As the centre has become better known its activities have expanded, and the 5 man team that started work in 1968 is now 30 strong. Turnover next year is expected to top £250,000.

NEW WORLD

Cancer Advisory Board Expresses Concern

by our Washington Correspondent

THE National Cancer Advisory Board, the chief advisory body to the National Cancer Institute, last week sharply criticized the Administration's plans to phase out NIH training grants and fellowships, and appealed directly to President Nixon to get them reinstated. Specifically, the board passed a resolution during its regular quarterly meeting requesting the President's Cancer Panel — a panel consisting of three members which reports directly to the President — to "seek a personal audience with the President" to bring the Board's concern to his attention.

The Administration's budget request for 1974 proposes that NIH training grants should be phased out, and provides no money for approving new grants. Only those grants which still have some time to run will be funded, and so eventually, if the Administration gets its way, the NIH will be forced entirely out of the business of giving direct support to graduate students.

The Administration supports its case by arguing that since research budgets are now "stabilized", demand for manpower will not grow as quickly as it has in the past few years, and there is danger of an oversupply of trained biologists. It has also been argued that since scientists who hold a PhD expect to earn large salaries, they should be able to finance their training by taking loans.

But the Administration's plans have been attacked vehemently by university scientists, who have complained that phasing out the training grants will choke off the supply of graduate students. Dr James Watson, Professor of Molecular Biology at Harvard and Director of the Cold Spring Harbor Laboratory, recently told the Senate Health Subcommittee, for example, that the policy is "lunacy". He pointed out that most important new discoveries in biology come from people under the age of 35 who are relatively unknown at the time they make their breakthrough, and so the shutting off of NIH training grants and fellowships, coupled with the trend towards contract research, will concentrate resources in the hands of middle aged entrepreneurs. Also the science will for the most part have to be done "by an age group not noted for working into the night".

Watson's statements to the Senate Health Subcommittee were also backed by Dr Lewis Thomas, Dean of Yale University School of Medicine and

President-elect of the Memorial Sloan Kettering Cancer Center. Both Watson and Thomas are, however, university scientists who represent the group most severely affected by the cutbacks. They could, therefore, be accused of special pleading. But earlier this week, the Carnegie Commission on Higher Education, in a report on the job outlook for graduates, reported that prospects are bright for graduates entering the health care professions, and also warned against any cutbacks in federal funds for training in health and allied professions.

CONGRESS

New Force in the Senate

by our Washington Correspondent

WHILE the Administration has been scrapping, revamping and—some say—downgrading its science policy machinery, Congress has been quietly building up its scientific strength. The latest move is the setting up of a subcommittee on Science, Technology and Commerce as part of the Senate Commerce Committee. Although the subcommittee's role is still being worked out, it has a broad mandate to study scientific and technological questions, and it seems that its work will to some extent parallel that of John Davis's subcommittee on Science, Research and Development in the House of Representatives.

The chairman of the new subcommittee is Senator John Tunney of California, and other members so far appointed are Adlai Stevenson III of Illinois and James Pearson from Kansas. Both Tunney and Stevenson were appointed to the Commerce Committee for the first time this year. It will be the first subcommittee in the Senate to have such a wide-ranging interest in science since the demise in 1969 of Senator Fred Harris's subcommittee on government research.

One of the subcommittee's chief concerns will be the workings of the National Bureau of Standards, which is part of the Department of Commerce, and in that respect it will handle all the legislation dealing with metric conversion. Fire safety research and development will also fall in the committee's purview. But Senator Warren G. Magnuson, chairman of the Commerce Committee, clearly sees the subcommittee as having a broader role than simply overseeing the affairs of the NBS, for he said last week that it would be concerned with the "taming and channel-

ling of technology towards the solution of national problems". An aide to Senator Tunney added that the subcommittee will also probably take a close look at investment in science and technology in relation to the US balance of payments.

Started in 1946, the NIH training and fellowship programmes grew steadily until last year, when the funding reached nearly \$190 million. This year, funding is being held at \$150 million, and it is set to go down next year to \$129 million. Moreover, general research support for every institute except the National Cancer Institute and the National Heart and Lung Institute is also set to decline, which in turn will mean that there will be less money available in the universities to support graduate students from the general project support grants.

An important factor is that Senator Tunney is a close personal and political friend of Senator Edward M. Kennedy, and cooperation between his subcommittee and Kennedy's subcommittee, which oversees the work of the National Science Foundation, is therefore to be expected. Such cooperation would, for example, facilitate any move to broaden Kennedy's National Science Policy and Priorities Act (S32) to provide a role for the National Bureau of Standards. In the House of Representatives, such jurisdictional problems would not arise because Davis's subcommittee has oversight over both the NSF and the National Bureau of Standards.

Congress's other new science unit, the Office of Technology Assessment, is still waiting for funds before it can officially get under way. A supplemental appropriations bill is not now expected to be passed until June or July at the earliest, and so appointment of staff and the initiation of studies will have to wait until the summer or autumn. There has also been a minor squabble among the members of the OTA board of directors appointed from the House of Representatives, about who should be the board's vice-chairman. Kennedy has already been elected chairman, and John Davis reckons that he should get the job of vice-chairman. But Charles Mosher, a Republican from Ohio and ranking minority member on Davis's subcommittee, reckons the job should go to a Republican, namely himself. And that is where the matter stood last week. The OTA board meets for the first time next week, and the office's advisory council may then be appointed.

The Administration argued when it phased out graduate support grants from the National Science Foundation that graduate students would be able to pick up their support from project grants, and it has also applied the same argument to the NIH training grants. But the National Cancer Advisory Board looks on that argument with considerable scepticism, for although its recommendation that training grants be reinstated in theory applies only to the National Cancer Institute, several board members are particularly concerned about the effect on the cancer programme of cutbacks in other biomedical sciences.

The resolution adopted last week specifically points out that the National Cancer Act of 1971 specifies that the board, together with the director of the National Cancer Institute, should provide a sufficient manpower base in fundamental sciences and clinical disciplines to carry out the cancer program. "The Board feels that it cannot carry out those responsibilities in the absence of authority to fund training programs", the resolution states.

If the board's appeal falls on deaf ears in the White House, which seems likely, it is unlikely to go unheeded in Congress—Kennedy, for one, will probably try to get the training programs reinstated. But, if the history of the National Science Foundation's graduate training programmes is anything to go by, the Administration will get its way in the end. For the past three years, Congress has directed the Administration to increase its proposed spending on NSF graduate support grants, but each year the Office of Management and Budget has simply impounded the extra money.

AIR POLLUTION

Reducing Hazards

by our Washington Correspondent

THE Environmental Protection Agency has at last set controls on the discharge of asbestos, beryllium and mercury into the atmosphere. Designed to protect public health, the controls apply to milling and manufacturing industries, the demolition of buildings and the burning of wastes. They have taken more than two years to develop—a year longer than the Clean Air Act allows—and the EPA reckons that they will cost industry a little more than \$50 million a year, with the chief cost falling on the demolition industry.

The three pollutants were chosen because they each have a potentially serious impact on public health, causing or at least contributing to increase in mortality, and serious and incapacitating illness. But, although the need for stringent control over their emission has been widely recognized, the EPA has had

considerable difficulty in drawing up suitable regulations.

As for asbestos, it is extremely ubiquitous, tiny asbestos fibres in the atmosphere constitute a serious health problem, and it is very difficult to regulate. The fibres lodge in the lung, causing a debilitating and often fatal condition among asbestos workers, known as asbestosis. They have also been linked with cancer of the lung and with a variety of other cancers. A widely publicized study by Dr Irving Selikoff of the Mount Sinai Medical School, for example, has come up with the prediction that about 95,000 of the estimated 250,000 asbestos workers in the United States will eventually die of cancer.

Such considerations led a committee of the National Academy of Sciences to recommend last year that stringent controls should be placed on the emission of asbestos fibres into the atmosphere, but the setting of such controls has been hampered by the fact that satisfactory means of measuring ambient asbestos concentrations have only recently been developed, and a method of measuring asbestos emissions is still unavailable. The EPA even considered banning production, processing and use of asbestos completely, but eventually decided that such a drastic step would lead to the banning of extremely important uses of the material.

In the event, the agency decided to set visible emissions standards for a number of operations, and to require that a specific procedure be followed in the demolition of buildings. In short, the regulations set last week specify that there shall be no visible emission of asbestos from asbestos mills or from the manufacture of any product which contains asbestos, such as cloth, floor tiles and insulating materials.

As for demolition of buildings containing asbestos products, the regulations specify that the material should be removed before the building is demolished, that it should be wetted before removal, and that the EPA should be notified at least 20 days before demolition takes place. The regulations apply to all buildings except homes and apartment buildings with four living units or less. The controls are expected to increase demolition costs by about 8 per cent.

The beryllium and mercury standards were a little easier to draw up, because emission of each pollutant can be measured. The beryllium regulations apply chiefly to extraction plants, foundries and ceramic manufacturing plants and they specify that the maximum daily release of beryllium into the atmosphere from a plant should be 10 grams. As for mercury, the regulations specify that no more than 2,300 grams of mercury vapour should be released from stationary sources.

BALLOON RESEARCH

Long Life Aloft

by our Washington Correspondent

AFTER making two complete orbits around the Earth in 36 days, an experimental balloon carrying about 90 pounds of scientific instruments was brought back to the ground within 10 miles of its launching site in Australia last month. A second balloon, launched at the end of January, is still aloft after being becalmed over the South Pacific, and it is expected to complete its second orbit later this month. The flights, which were carried out as part of NASA's balloon research programme, represent the first successful orbiting of scientific instruments by balloon, and they are an important step on the way to development of a balloon capable of orbiting up to about 500 pounds of instruments at 130,000 feet, and of staying aloft for six months or more.

Although the balloons carried scientific instruments—cosmic ray detectors and micrometeorite collectors—the chief objective of the flights was to test the performance of a new type of balloon. Called super pressure balloons, they are designed to fly at an altitude determined by constant atmospheric density, and unlike conventional balloons, they have no valve for venting gas when excess pressure builds up. Consequently, the skin must withstand changes in pressure caused by temperature changes between night and day, and it must also be resistant to degradation by ultraviolet light and fast moving micrometeorite particles.

Conventional balloons are usually inflated on the ground and lift the payload to the required height. As the altitude increases, the balloon expands and excess pressure is vented from a valve at the base, but at night, when the temperature drops, the balloon shrinks and descends. The skin is, however, not completely elastic, and when the balloon expands again during the day, it vents some more gas. Consequently, conventional balloon flights provide only a few hours worth of scientific results.

Super pressure balloons, on the other hand, are constructed of material which can withstand the excess pressure developed by changes in temperature, and they are also designed to be resistant to degradation by strong ultraviolet light and fast moving dust particles in the upper atmosphere. The two balloons which NASA has been orbiting will now be carefully studied for signs of wear, and if they have survived their flight successfully, the prospects for lifting payloads to 130,000 feet for several months seem very good.

If NASA's objective is attained, super pressure balloons would open up considerable new possibilities for upper

atmospheric studies and for cosmic ray research. Although even at that height, atmospheric effects would interfere with X-ray and gamma-ray astronomy, Dr Carl Fichtel of NASA's Goddard Space-flight Center, said last week that long and reliable balloon flights could be "extremely valuable" for low flux events, such as high energy electron-positron studies.

As for the two test flights, the balloons were carried to a height of about 78,000 feet by a small conventional balloon, and then released. They were carried by the air stream in an orbit passing over Australia, Africa and South America. The original intention was to bring the first balloon down after one orbit, but weather conditions at the landing site were not good, and it was decided to leave it to make another orbit. And, now that the second balloon has become becalmed, NASA will be able to look at the effects on the skin of at least 60 days exposure to intense ultraviolet radiation and micrometeorites.

PESTICIDES

Firing the Ant

by our Washington Correspondent

THE Department of Agriculture's long war against the imported fire ant — an insect with a painful sting which has established itself in the Southern United States—will move to a new battleground this summer. The Environmental Protection Agency has called a number of public hearings to help it decide whether a controversial pesticide known as Mirex should continue to be used against the ant. The hearings may finally put an end to the considerable controversy which has surrounded the Department of Agriculture's campaign against the ant in general, and the use of Mirex in particular, and a decision will be made before August 15, when the autumn treatment programme is due to begin.

First introduced into the United States from South America in the 1920s, the fire ant has since spread through the south-eastern states, from Texas to North Carolina. Although it does little or no direct damage to crops, the ant has been treated to an onslaught of pesticides in campaigns financed partly by the US Department of Agriculture (USDA) because of its painful sting and because its mounds damage agricultural machinery. The USDA programme at first attempted to eradicate the ant, but recently has concentrated on trying to prevent it from spreading any further.

The campaign against the ant has, however, been attacked by environmentalist groups and by several scientists, partly on the grounds that it is not worth the expense, but also because the chief weapon, Mirex, persists in the environment, tends to collect in some

animals and insects and has been found to cause cancer in mice. In March, 1971, William D. Ruckelshaus, Administrator of the Environmental Protection Agency, decided to cancel registration of Mirex because of the doubts about its safety, but the chief manufacturer, Allied Chemical Corporation appealed the decision and asked for review of the pesticide by an independent scientific advisory committee. The committee eventually gave qualified approval to Mirex, and in July last year, Ruckelshaus lifted the cancellation order but forbade use of the pesticide near water or heavily forested areas, and also stopped application from aircraft in coastal regions. The hearings this summer will re-examine those decisions.

The scientific advisory committee which examined the use of Mirex last year (see *Nature*, **235**, 353; 1972), concluded that the ant should be controlled because its presence restricts use of recreational areas, and because it is continuing to spread. The committee also concluded that Mirex is effective in controlling the ant, that it is not acutely toxic to man, and that it does not seem to present much of a long term hazard although more chronic toxicity studies are urgently needed. Mirex residues have, however, been found in relatively high concentrations in aquatic animals, and the committee consequently recommended that it should be kept away from water.

The pesticide is applied in small doses in a bait which the fire ant takes back to the mound, and it then wipes out the whole ant colony. When the Mirex treatment campaign first started about 15 years ago, the Department of Agriculture pumped more and more money into it, and was egged on by Congressional agriculture committees. But as the ant continued to spread, it became clear that complete eradication was impossible, and the USDA has recently been concentrating on preventing the ant from spreading. This year the USDA intends to treat about 20 million acres out of a total infested area of about 126 million.

Short Notes

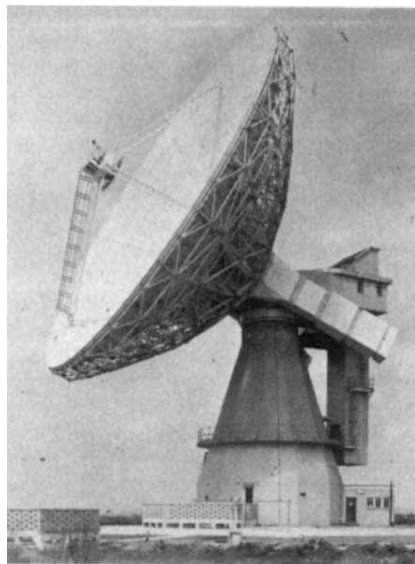
Lunar Studies

THE National Aeronautics and Space Administration has announced that it has established a new programme for analysing lunar data derived from the Apollo programme. Called the Lunar Data Analysis and Synthesis Program, it will encourage multidisciplinary studies which draw on data from a number of sources, such as Apollo experiments and photography, lunar mapping, Moon samples and theoretical studies and Earth-based observations. The lunar programme is now part of the Office of

Space Science, and the Lunar Programs Division is headed by William T. O'Bryant.

Second Biggest

KITT Peak National Observatory, atop a sacred mountain of the American Indians of southern Arizona, will soon have the second largest telescope in the world. The 158-inch Mayall Telescope, named after a director of the observatory, is now undergoing final tests before observations start in October.



Financed at a cost of \$10 million by the National Science Foundation, the telescope is housed in this building designed by the Chicago office of the Skidmore, Owings and Merrill architectural firm which also designed the visually striking McMath Solar Telescope also on Kitt Peak.

Weather Modification

A RESOLUTION was introduced into the House of Representatives last week by Mr Don Fraser of Minnesota calling on the United States to seek agreement with other members of the United Nations on a treaty banning "research, experimentation or use of weather modification activity as a weapon of war". A similar resolution was earlier introduced into the Senate by Senator Pell.

Psychosurgery

A TWO-YEAR moratorium on federal projects involving psychosurgery was called for last week by three Republican Senators. Senators Beall of Maryland, Dominick of Colorado and Buckley of New York introduced a bill into the Senate calling for such a moratorium and also directing the Secretary of Health, Education and Welfare to conduct a comprehensive study of psychosurgery and to recommend within a year, what circumstances, if any, justify its use.

NEWS AND VIEWS

Record QSO Redshift Observed

ALMOST exactly ten years ago, on March 16, 1963, the story of quasistellar objects (QSOs) began in the pages of *Nature* (197, 1037) with the announcement of the identification of the radio source 3C 273 with a thirteenth magnitude stellar object with a redshift (z) of 0.158. Anybody following, even casually, this continuing story must by now be acquainted with some of their chief features. QSOs are now recognized as being associated with about 25 per cent of the known radio sources. Using direct photography most of those so far identified are difficult to distinguish from normal galactic blue stars although they differ from such stars in having a characteristic ultraviolet excess; that is, they not only appear brighter on the blue O-plate of the Palomar Sky Survey than on the red E-plate but also appear brighter on a plate taken with an ultraviolet (U) filter than would a galactic star which appears equally bright using a blue (B) filter. A conclusive identification, however, requires a spectroscopic analysis to show the characteristic broad emission lines (among them and typically the strongest Lyman α , CIV, CIII, MgII and H α) which are shifted towards the red by an amount greatly in excess of that observed in extragalactic nebulae of comparable apparent magnitude.

Immediately following the discovery of QSOs it was realized that on the conventional interpretation of the redshift in terms of the Doppler effect associated with the expansion of the Universe and related to distance by Hubble's law, QSOs provided the most powerful tool available for the investigation of the geometry and history of the Universe. Using even the brightest radio galaxies it was clear that optical observations could be extended to redshifts greater than 0.5 only with great difficulty, whereas with QSOs such investigations could be contemplated for objects with $z \geq 2$ where the differences between various cosmological models becomes large. With the cosmological problem in mind strenuous efforts were therefore made to detect more and more QSOs, particularly QSOs with large redshifts. These searches were carried out in two ways: optically, using U and B filters first to detect objects with the characteristic ultraviolet excess, and by using lists of radio positions to locate blue stellar objects on the Palomar Sky Survey plates (radio positions were in general not sufficiently accurate to permit a reliable identification on the basis of positional agreement alone). Progress was at first rapid and by 1966 some 100 redshifts, some as large as $z \approx 2.2$, had been measured. Since then progress has been significantly slower and only two or three objects with larger values have been reported, the record ($z = 2.877$) being held by an eighteenth magnitude QSO associated with the radio source 4C 05.34 (Lynds and Wills, *Nature*, 226, 532; 1970). As this QSO is well above the magnitude limit of the Sky Survey plates and noticeably blue, Lynds and Wills considered that there could be no significant selection effects preventing the discovery of such objects up to $z = 3$ and therefore that the low rate of discovery of objects with high redshifts must be a consequence of their intrinsic rarity. An essentially similar conclusion has been reached by Schmidt (*Astrophys. J.*,

176, 273; 1972) who analysed an optically selected sample of QSOs and found that the distribution of redshifts could only be explained if the density of QSOs (per unit of comoving volume) increases approximately as $(1+z)^6$ up to a redshift of approximately 2, but then decreases, or at least increases much less rapidly.

This conclusion raises the interesting possibility that a redshift of 2 represents an important epoch in the evolution of the Universe but is somewhat disappointing in that it indicates a degree of density evolution that is certainly sufficient to mask the geometrical effects expected from different model universes. If this is indeed the case it may still be possible to distinguish between different cosmologies using a method first suggested by Hoyle, who pointed out that in most cosmological models a source of given linear size reaches a minimum apparent angular size at $z \approx 1$, beyond which the apparent size for various models rapidly diverges. No such minimum has yet been detected but here again evolutionary effects may be important and it may be necessary to go to redshifts greatly in excess of 2 before the geometrical effects become apparent.

Cosmologists will therefore be elated by the report from Carswell and Strittmatter on page 394 of this issue of *Nature* that the barrier at $z = 3$ has at last been crashed. Of particular interest is the fact that it has been observed on the basis of positional agreement alone and without the help of the usual selection criteria, thus raising the question whether, up to now, these selection criteria have been militating against the detection of high redshifts.

It has long been known that at least the magnitude of the ultraviolet excess is dependent on redshift (Strittmatter and Burbidge, *Astrophys. J.*, 147, 13; 1970), largely as a result of the passage of the strong emission lines through the relatively narrow U filter. In particular there is little doubt that the ease with which redshifts around 2 have been measured is attributable to the shift of the strong Lyman α at this redshift from its rest wavelength of 1,216 Å to 3,648 Å, near the peak response of the U filter. For a redshift range $z = 2.2$ to 3, however, Lyman α appears instead in the B filter with the result that the ultraviolet excess may disappear although the object may still appear blue. Browne and McEwan (*Nature*, 239, 101; 1972) have noted a number of objects which have these characteristics. The sharp decrease in the continuum at wavelengths shorter than 4,000 Å noted by Carswell and Strittmatter certainly shows that QSOs with $z > 3.4$ will not be characterized by an ultraviolet excess. This is not to say that high redshifts may not indeed be rare, simply that they will be difficult to detect by optical search techniques. It would seem that a significant extension of the observations to redshifts greatly in excess of 3 will therefore be difficult on account of the lack of an ultraviolet excess.

Positions accurate to about 1 arc s will be required, for it is clear from the identification of OH 471 that the usual blue criterion will not be applicable. Indeed it may be worthwhile to reverse this criterion and concentrate on very red objects. Thus a QSO with a redshift of $z \approx 4$ will have Lyman α shifted to the peak response of the

E-plate and with the form of the continuum spectrum described by Carswell and Strittmatter may appear very red indeed. Furthermore Lyman α is so strong relative to the continuum that its presence may more than compensate for the increase in distance so that a QSO with $z \approx 4$ may be even brighter than the eighteenth magnitude object associated with OH 471. The detection of objects with even higher redshifts is clearly feasible particularly if plates are used with a peak response significantly to the red of the Palomar E-plate.

The observation of Carswell and Strittmatter thus holds out hope that with radio positions accurate to 1 arc s becoming routinely available, objects will be identified with sufficiently high redshifts to allow a separation of the geometrical effects of different cosmologies from evolutionary effects. They have shown that although these high redshift objects may be rare they do exist. C. H.

Marine Conservation

SUCH has been the success of marine conservation areas in the several countries which have set aside sublittoral areas for total protection or controlled exploitation, that it was inevitable that pressure would mount for a conservation policy for marine habitats in the British Isles. A working party on marine wildlife conservation set up by the Natural Environment Research Council has recently produced an assessment of evidence of the threat to marine wildlife and the need for conservation measures (*Marine Wildlife Conservation*, NERC Publ. Series B, No. 5, 39 pp; 1973). The working party, under Professor R. B. Clarke, reports that some 461,000 acres of coastal habitat—consisting of mud flats, marsh, cliffs, sand dunes and shingle—are already conserved within National Nature Reserves, but that most of these exist not because of their littoral importance but because they have ornithological or physiographical interest. A substantial area of marine habitat is thus already partly protected more by accident than design, a phenomenon attributable to the terrestrial base of the Nature Conservancy.

One of the priorities of the working party was to establish exactly what changes had taken place in the marine flora and fauna and the factors involved in the changes; a substantial part of the report is concerned with this evidence. A second priority was to examine the current marine conservation measures in the United Kingdom. Recommendations for action should clearly stem from both.

The difficulty in practise is that recommendations can only stem from a position of knowledge, and it seems from the evidence presented that the facts concerning change in status of marine organisms are acutely inadequate. Even the report enters the caveat "that the evidence we received was largely circumstantial, relates to comparatively few areas, and came from relatively few sources". Evidence of changes were received from members of staff of marine laboratories and fisheries departments, local authorities, sea fishery committees, conservation societies and other interested organizations. Given the necessity of gathering information by questionnaire, the variety of interest here could be sure to reveal some bonnets buzzing loudly with bees.

Analysis of the evidence presented, in which changes

of abundance are related to the causative factor, produces an interesting league table:

Skin Divers	26	Climatic change	14
Pollution	9	Bait digging	9
Unknown	21	Others	2

Skin divers are thus clear favourites. Most of the reported consequences of their activities refer to local exploitation of stocks of commercial shellfish—crabs, lobsters, crawfish especially, and scallops. Heavy collecting of the sea fan, *Eunicella* and the sea urchin, *Echinus*, for sale to tourists in the major holiday resorts, is also reported. The coasts of Dorset, Cornwall, the Isles of Scilly and West Wales are particularly affected, but this is because these areas are suitable for diving. Many other areas contain these species in abundance, but because of inaccessibility, or cloudy water, are unsuitable for diving which suggests that over exploitation will never be more than a local phenomenon.

Bait digging in intertidal areas is an unexpected hazard to the marine biota but is certainly over-emphasized in the analysis. In fact, eight out of the nine cases of reported decline are concerned with marine worms and most of these reports stem from a single observer and locality. As the working party's report points out, disturbance of the foreshore by bait digging may expose other invertebrates and lead to their decline—but no evidence is produced to quantify this effect. Moreover only four widely distributed areas are named where damage from bait digging is reported.

Decline in numbers as a result of pollution is also reported, especially among molluscs, filter feeders and algae in heavily polluted areas such as the Mersey mouth and the Durham coast. Sea anglers too are said to have had reduced success in the Bristol Channel, Solent and Weymouth areas, a report "which is authenticated by club records". "Authenticated" might seem a strong word for evidence from angling club records. Most anglers attribute their lack of success to factors beyond their control, and "pollution" is a convenient and fashionable whipping boy.

Two other principal causes of change in the marine fauna are "unknown" (records for which mostly stem from professional marine biologists) and climatic changes (which have been for long well documented in the scientific literature).

These examples are symptomatic of much of the evidence showing that the marine fauna and flora has changed appreciably in the recent past. In essence they suggest that the most important factor causing changes in the marine environment is climatic, which has long been appreciated and studied. The remaining evidence may suggest local trends which require quantitative assessment but more likely to be attributable to the totally subjective nature of much of the evidence.

The working party's recommendations to NERC, that in view of the paucity of factual information long-term studies into natural population fluctuations be intensified and that sources of information and advice should be identified and involved in future conservation measures, are clearly unavoidable and highly desirable. The setting aside of littoral and sublittoral areas as conservation reserves equally clearly can wait until the need for them is established by quantitative evidence gathered by objective survey.

From a Correspondent

FOETAL PHARMACOLOGY

Drugs and the Unborn Child

by our Special Correspondent

SOME pregnant women are probably consuming drugs unnecessarily, to the detriment of their future offspring. This warning from several speakers at a symposium in New York on March 15 and 16 was underlined by the revelation of general ignorance of many of the effects of drugs on the human foetus. The occasion was a gathering of representatives of all disciplines associated with human development, brought together by the March of Dimes to discuss recent advances and unsolved problems concerning drugs and the unborn child. As the meeting progressed, however, unsolved problems predominated the proceedings, and it became clear that it is by no means simple to identify a particular drug as a potential hazard to the foetus.

The voracity of pregnant women for drugs is well illustrated by a retrospective study of 911 women in Edinburgh, reported by Dr J. O. Forfar (University of Edinburgh). His team found that, excluding iron, drugs were prescribed for 82 per cent of the women during pregnancy, with an average of four drugs each. Sixty-five per cent of the women took non-prescription drugs, mostly aspirin and antacids. The survey revealed a tendency to administer certain drugs—antibiotics, cough medicine, iron and tranquillizers—throughout pregnancy; others—anti-emetics, antihistamines, appetite suppressants, bronchodilators and hormones—early in pregnancy; and others—for example, antacids, analgesics, barbiturates, diuretics and hypnotics—late in pregnancy. Dr Forfar's conclusion that the basis on which many of these drugs were used could be questioned, was echoed by Dr L. Stern (McGill University). He wondered whether, for example, all the pregnant women who took thalidomide, and later gave birth to severely malformed babies, had really needed the drug as treatment for insomnia.

Other speakers urged great care in the prescribing of drugs during pregnancy. But the situation for the physician is often not clear cut, as was explained by Dr L. M. Hellman (US Department of Health, Education and Welfare). The value of a drug must be measured according to the ratio of benefit to safety, which makes it very difficult for anybody to rule that a particular drug should never be given to pregnant women. For example, some attention at the symposium focused on diphenylhydantoin ('Dilantin'), which is suspected sometimes to cause birth defects such as cleft palate. But 'Dilantin' is an

important anti-convulsant drug, which is given to pregnant women as a treatment for epilepsy. As epileptic seizures must obviously be avoided during pregnancy it would be rash immediately to ban or restrict the use of this drug.

Faced with such problems, the physicians turn for information to the experimentalists, who are beset with problems of their own. The effects of drugs on the foetus can only be assessed in comparison with the normal sequence of changes involved in biochemical development. At present little is known about the situation in the human foetus, and so it was not surprising that contributors to the biochemical sessions of the meeting concentrated on reviewing data obtained with rodents and other animals. Dr O. Greengard (Harvard University), for example, reported progress with the rat, revealing that four different groups of enzymes, induced by various hormones, appear at four different stages

of foetal development. Unfortunately, as data slowly amass, it becomes increasingly clear that the human foetus, far from resembling the rat, is biochemically unique in its development. Dr D. B. Villee (Harvard University) reported that the biosynthetic pathway of the steroid hormones in the human develops quite differently from that in other species. This underlines one of the greatest problems of students of drug metabolism—the paucity of suitable animal models that can be trusted to parallel the situation in the human foetus. The baboon, however, offers some hope as a model for the study of foetal steroid metabolism, according to Dr S. Solomon (McGill University).

Summarizing the many goals of developmental pharmacologists, Dr B. L. Mirkin (University of Minnesota) gave highest priority to the study of the passage of drugs into the foetal environment throughout gestation; the effects of drugs on normal development; the influence of postnatal maturation on drug disposition; and inherited and acquired factors influencing the action of drugs. Increased

A Common Receptor for Hallucinogens?

It is always the hope that elucidating the mechanisms of hallucinogenic drugs will give clues as to the nature of mental disorders and ways of developing treatment. Psychopharmacologists would no doubt be delighted to find that hallucinogens operate by way of a common effect.

Szent-György's group have studied several psychoactive compounds and suggested that their action might result from their powerful ability to donate electrons (*Science*, **130**, 1191; 1960). Snyder and his co-workers have found a high correlation between the potency of hallucinogens and their willingness to give up electrons (*Proc. US Nat. Acad. Sci.*, **54**, 258; 1965). Three dissimilar classes of drugs have been found to produce psychedelic effects—lysergic acid diethylamide (LSD), derivatives of tryptamine, and compounds related to phenylethylamine. Theoretical calculations showed that within each class, the agents which can best donate electrons are those with the greatest hallucinogenic effect in man. A later study showed that the psychoactive drugs in each group are capable of assuming conformations analogous to some part of the ring structures of LSD (*Proc. US Nat. Acad. Sci.*, **60**, 206; 1968).

Some experimental support for these theoretical studies has been provided by Smythies *et al.* (*Nature*, **226**, 644; 1970), who found that THPC (which represents the D ring of LSD) will antagonize the behavioural effects of LSD, but greatly potentiates the effect of mescaline, which is a phenyl-

ethylamine resembling the A and B ring portion of LSD. This suggests that mescaline occupies part of the site acted on by LSD, but that the complete structure is necessary for the full effect.

In the forthcoming issue of *Nature New Biology* (April 11), Smythies's team now report that behavioural effects of two amphetamine derivatives support the notion of a common site of action. Using an animal behaviour task (Sidman avoidance), they compared the psychomimetic activity of stereoisomers of 2,5 dimethoxy-4-bromoamphetamine (DOB) and 2,5 dimethoxy-4-methylamphetamine (DOM). Their reasoning was that if these two amphetamine derivatives, which show profound psychoactivity in man, act by way of a LSD-type receptor, then their R(−) isomers should have the greater effect because the R(−) conformation corresponds to that of LSD. The stereospecific approach is powerful because it controls for variations in the transport and metabolism of drugs. Presumably, only specific receptor sites would differ in their response to the subtle steric differences between the two isomers. In these experiments, the R(−) isomers of both DOM and DOB were considerably more active than the alternate S(+) configuration.

If subsequent investigations continue to support the notion of a common receptor for hallucinogenic drugs, then the central question will be: what is the endogenous neurochemical which is the true substrate for the "hallucinogenic receptor"?

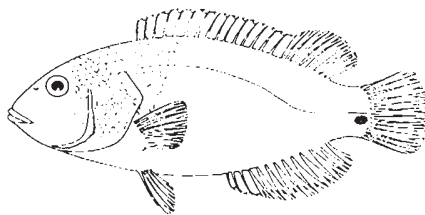
fundamental knowledge of this kind may bring not only the ability to assess potential harm to the foetus from drugs consumed by pregnant women, but also the opportunity to practise foetal therapeutics. Dr R. A. Chez (National Institute of Child Health and Development, Bethesda) looked forward to further procedures such as those now being developed to treat, for example, foetal erythroblastosis and respiratory distress in newborn infants through medication of the mother before birth.

WRASSES

Cleaning Symbioses

from our Marine Vertebrate Correspondent
THE discovery that some fishes enter into symbiotic relationships with others whereby they clean their hosts of parasitic and fungal infections was essentially a by-product of the development of compact underwater breathing apparatus. The freedom this gave to biologists to observe fish behaviour untrammelled by captivity quickly led to several observations of fishes, especially wrasses (family Labridae), which specialize in this symbiotic relationship. Many cases have been reported in tropical seas but recent studies show it to occur in temperate seas, as off New Zealand (see *Nature*, **238**, 433; 1972) and in European freshwater fishes (E. F. Abel, *Oecologia* (Berl.), **6**, 133; 1971). More recently G. W. Potts (*J. Mar. Biol. Ass. UK*, **53**, 1; 1973) has found that several north European marine fishes may be involved in cleaning symbioses.

Potts reports observing the shallow water wrasse *Centrolabrus exoletus* pecking at the sides of a ballan wrasse *Labrus bergylta* posturing in the fashion adopted by host fishes soliciting the attention of a cleaner. Three wrasses were inspected and cleaned by this *Centrolabrus* specimen, operating in a shallow sublittoral region on the Devon coast. Potts also reports cleaning behaviour by several species of wrasse, and other species, in the tanks of the public aquarium at the Plymouth Laboratory. The species involved were the goldsinny, *Ctenolabrus rupestris*, and the corkwing wrasse, *Crenilabrus melops*. Free-living specimens of the latter from the Plymouth region have been found to contain in their stomachs the externally parasitic praniza larvae of certain crustaceans, which suggests that cleaning symbiosis is part of the way of life of this species. In the aquarium *Crenilabrus melops* became active soon after the aquarium lights were switched on each morning and approached various fishes which responded by invitation postures. Many of the host fish carried the praniza larvae of the crustacean *Gnathia maxillaris*, adults of which live in the gravel of the



Corkwing wrasse, *Crenilabrus melops*.

aquaria. The wrasse were evidently removing these nocturnally-active parasitic larvae from their hosts.

It is doubtful, however, to what extent Potts's observations are referable to natural conditions. The fish associations of this aquarium—mackerel, black sea bream, red sea bream, plaice, ballan wrasse and corkwing wrasse are mentioned — are unlikely to be found in nature. The evident abundance of the praniza larvae of *Gnathia maxillaris* is also possibly an artefact of aquarium life. If anything, these observations suggest that even in temperate waters some wrasse species have a tendency to enter into a symbiotic cleaning relationship with other fishes. In free-living conditions this tendency may well remain mostly latent, but in the aquarium the adaptable, and opportunist, feeders such as *Crenilabrus* and *Centrolabrus* may take advantage of the opportunity to clean other fishes.

It is noticeable that most of the cleaners described among tropical (and the New Zealand) wrasses have contrasting dark markings, a condition which has led to the suggestion that cleaner fishes have distinctive "guild mark" coloration. Both the temperate European species which Potts has shown can act as cleaners have distinctive dark markings on the sides of the body. Other wrasses in British waters have similar dark contrasting markings either during their juvenile stage or throughout life. It would be worthwhile investigating whether they too act as cleaners, for whether they too act as cleaners.

TUMOUR VIRUSES

Origin of New Isolate

from our Cell Biology Correspondent
RD114 virus is a C-type RNA virus which contains 60–70S single stranded RNA and reverse transcriptase. It is liberated from RD114 cells which have a human karyotype and which were derived from a tumour induced in a kitten by inoculations of RD cells. RD cells were in turn derived from a human rhabdomyosarcoma; they have been adapted to continuous growth in culture and they do not liberate detectable C-type particles. This is, of course, a familiar story to anybody who has followed the search for human RNA tumour viruses, and the question of whether or not RD114 virus is of human origin or a feline virus or even some

recombinant human/feline virus is still open. What is known is that the antigens and reverse transcriptase of RD114 virus are not related either to the antigens and enzyme of feline leukaemia and sarcoma viruses nor to the antigens and enzyme of primate (woolly monkey and gibbon) C-type RNA viruses.

In an attempt to throw light on the question of the origin of RD114 virus Todaro *et al.* (*Proc. US Nat. Acad. Sci.*, **70**, 859; 1973) obtained RD cells from McAllister who, with his colleagues, isolated them as well as RD114 cells and RD114 virus and transplanted them into NIH Swiss mice that had been immunosuppressed with antithymocyte serum (AT). The immunosuppression, of course, allowed the human RD cells to grow in a heterologous species. The tumours that developed in immunosuppressed NIH Swiss mice inoculated with RD cells could be explanted and grown in culture or passaged in further mice.

Todaro found that a cell line established from a third generation transplant tumour liberated a C-type virus. RD cells as noted earlier do not liberate C-type viruses, and NIH Swiss mice have a low incidence of leukaemia and have never been reported to produce C-type viruses even though some viral information is expressed. What then is the origin of this virus, called AT124, which is liberated by RD cells after passage in NIH Swiss mice and how if at all, is it related to RD114 virus?

AT124 virus proved to have a host range comparable with that of RD114 virus. Both viruses grow well in cat, primate and human cells but fail to grow in murine cells including NIH/3T3 cells. AT124 virus, however, contains murine virus reverse transcriptase, or to be more precise reverse transcriptase sensitive to inhibition by antiserum raised against murine reverse transcriptase, whereas the reverse transcriptase of RD114 virus is not related by any known criteria to feline virus reverse transcriptase. AT124 virus particles also contain the group specific antigenicity unique to murine C-type RNA viruses, whereas RD114 virus particles do not contain the corresponding feline antigenicity. Finally AT124 virus can act as helper for the replication of murine sarcoma virus.

These data clearly indicate that AT124 virus is quite closely related to the murine leukaemia viruses; indeed it is possible that AT124 virus will prove to be an endogenous virus of NIH Swiss mice. A second explanation of the origin of AT124 virus is, however, more attractive. Todaro and his colleagues found that ten other sorts of human tumour cells could be transplanted into NIH Swiss mice but none of the tumours that arose in the mice liberated C-type virus. Neither do chronically immunosuppressed NIH Swiss mice liberate C-type particles. These observations in-

dicating that the human RD cells contribute some function which allows the complete replication of C-type viruses. One can speculate for example, that cells of NIH Swiss mice contain an endogenous but replication-defective viral genome that by recombination with some part of the RD cell genome acquires the ability to replicate in RD cells.

Obviously there are other ways of explaining the origin of AT124 virus, but it should be possible to decide between the various alternatives by comparing the nucleic acids and antigens of RD114 virus, AT124 virus, primate, feline and murine leukaemia viruses and the endogenous C-type viruses that can be induced from primate, feline and murine cells by chemical and physical mutagens and carcinogens. Currently the betting seems to be that both RD 114 virus and AT124 virus will prove to be recombinant viruses derived from human and, respectively, feline and murine C-type virus genetic information.

AMPHIBIA

Centenarian Triton

from our Soviet Correspondent

THE alleged discovery in the Kolya region of a Siberian triton, a form of newt, which had apparently hibernated in a block of ice, and came to life when the ice was melted was at first treated with considerable scepticism by Soviet biologists. These doubts, according to a report in *Izvestiya* (No. 66, 1973) have now been allayed, and a long hibernation of the order of a century—some ten to fifteen times the triton's normal lifespan—is now accepted.

The discovery, at the depth of 11 m, of the triton in its ice was reported to geologist D. Kolomiitsev, who was working in the area, and who communicated the news to N. Shcherbak, head of the Zoological Museum of the Ukrainian Academy of Sciences. Shcherbak agreed to study the triton, which was dispatched to Kiev, still alive, in an improvised container consisting of a moss-lined briefcase.

Superficial inspection showed the triton to be of a brownish colour, with a bare glistening skin, and to have a considerable aversion to light. Detailed radiocarbon studies, made by Shcherbak and his colleague N. Kovalyuk from the Ukrainian Institute of Geochemistry and Mineral Physics, gave the triton's absolute age, calibrated against readings for several other animals of known ages, of close to a century.

Asked to comment on the significance of the discovery, Shcherbak was non-committal. He stressed, however, that it raises extremely interesting questions related to the conservation of living tissues and of the whole organism.

MESSENGER RNA

Thirteenth Stroke

from our Molecular Biology Correspondent

JUST as it is only the thirteenth stroke of the clock that casts doubt on the validity of all that has gone before, the body of evidence amassed in recent years on the affinity between mitochondria and bacteria receives a bad shaking from new revelations of Perlman, Abelson and Penman (*Proc. US Nat. Acad. Sci.*, **70**, 350; 1973). The identification of mitochondria with primaevial symbiotic bacteria is based primarily on features of the mechanism of protein synthesis. The arguments, it must be owned, are strong: in particular the ribosomes are small (at 60S actually smaller than those of bacteria), initiation of protein synthesis is thought to require a formylmethionine tRNA as in bacteria (or at least formylmethionine has been found in the mitochondrion), and response to the inhibitors, cycloheximide and chloramphenicol, is of the kind expected of bacterial rather than eukaryotic systems. Perlman *et al.* have now isolated the messenger RNA from mitochondria of HeLa cells and were evidently taken aback to find that it contains a tract of polyadenylic acid, a feature absent from bacterial messengers, but characteristic of those of animal cells.

The key to the success of this work was the use of the drug camptothecin, which arrests the synthesis of high-molecular weight nuclear RNA, but does not disturb the activity of the mitochondria. Radioactive label then enters only the mitochondrial RNA, as confirmed by the disappearance of label under the action of ethidium bromide, which is

again specific for mitochondrial RNA synthesis under the right conditions. Moreover this labelled RNA adheres to the membrane fraction, and is not susceptible to degradation by pancreatic ribonuclease until the mitochondrial membrane is disrupted. The radioactive profile, after gel electrophoresis of the RNA, shows two peaks corresponding to the larger and smaller ribosomal components, and a polydisperse spread of other species. Chains with a sufficiently long tract of polyadenylic acid can be caught on filters containing immobilized polyuridylic acid, and retention of mitochondrial RNA on such matrices was demonstrated. Isolation of the poly (A) segment by treatment of the RNA with pancreatic and T_1 ribonucleases, followed by gel electrophoresis, shows that the molecular weight is rather low, being estimated to correspond to some 50–80 residues, compared with perhaps 200 in cytoplasmic messengers. That the poly (A) is indeed associated with mitochondrial messenger is shown by its sedimentation with the polysome fraction, from which it is liberated by addition of puromycin.

Because the mitochondrial messenger never leaves the mitochondrion, the observations tend also to explode the hypothesis that the poly (A) acts as a carrier during the peregrination of the messenger from nucleus to cytoplasm. It is possible of course that the poly (A) segments may have more than one function, and Perlman *et al.* note that the mitochondrial messenger has the short length of its poly (A) in common with two viral RNAs: here again no transport process precedes utilization. A short poly (A) run has also now been found in a messenger RNA by

Replication of Poliovirus RNA

MANY, perhaps most, messenger RNAs that are translated in mammalian cells contain polyadenylate (poly (A)) tracts and in poliovirus particles the single stranded RNA, which serves both as messenger and as genome, is no exception. It has a 3'-terminal poly (A) tract and, according to experiments reported by Yogo and Wimmer in *Nature New Biology* next Wednesday (April 11), this poly (A) is probably transcribed from poly (U) sequences in the minus strand of the polio replicative intermediate which may be a circular structure.

Yogo and Wimmer showed that the plus strand RNA of the linear replicative intermediate has a 3'-terminal poly (A) tract longer than the poly (A) tract of RNA from virions. Furthermore, at the 3' end of the complementary minus strand there is a tract of poly (U). The linear replicative intermediate must therefore be a double stranded molecule with single strand 3' tails at opposite

ends, the plus strand tail being poly (A) and the minus strand tail being poly (U). Clearly a 3'-poly (U) tract on the minus strand cannot act as template for a 3'-poly (A) tract on the plus strand. The minus strand poly (U) must be 5' terminal to act as template for the 3'-plus strand poly (A).

To resolve this dilemma Yogo and Wimmer speculate that the replicative intermediate molecule may be circular. They suggest that during replication of poliovirus RNA the infecting parental plus strand acts as template for the synthesis of a complementary minus strand with 5' poly (U). They further suggest that the minus strand is circularized by a putative RNA ligase before it acts as template for synthesis of progeny plus strands with 3'-terminal poly (A) and that breakage of this circular replicative form could yield linear minus strands with 3' rather than 5' poly (U) tracts.

McLaughlin *et al.* (*J. Biol. Chem.*, **248**, 1466; 1973), and this is a case of particular interest, for it concerns the primitive eukaryote, yeast. The appearance of poly (A) in this cell suggests that it may be a common feature of all eukaryotes, and not only mammalian cells (though the absence of poly (A) from messenger specific for histones has been reported).

McLaughlin *et al.* followed the incorporation of label into the RNA, and have isolated the poly (A), after nuclease digestion of the rest of the chain, by adsorption on poly (T) coupled to cellulose. As a pretty demonstration that the poly (A) is a part of the messenger they have used a temperature-sensitive mutant, and shown that under nonpermissive conditions at 36° C the adenine label, previously incorporated at 23° C, is displaced from the poly-somes and sediments with free ribosomes. Moreover after dissociation of poly (A)-containing polysomes, all the poly (A) is found with the polydisperse messenger fraction. Another facet of this work is the application of a superior method of molecular weight estimation. Rather than merely use a sedimentation or electrophoresis procedure, with the hope that a calibration based on the usual RNAs will apply, they have performed end-group determinations. Chromatography of the product of alkaline hydrolysis, and comparison of the counts in adenylic acid and adenosine, showed in the first place that the poly (A) run was at the 3' end of the RNA as in other eukaryotic messengers, and that the chain length was of the order of fifty nucleotides. An interesting additional observation is the failure of the entire messenger fraction to bind to the poly (T), and the absence of poly (A) from the part that does not bind. The integrity of the ribosomal RNA components indicates that this is not the result of nuclease activity, and it may present a parallel to the case of histone messenger. The variety of possible roles for the poly (A) tract of the messenger is now a subject of speculation, and no doubt new experimentation.

A variant of the end-group method has been used to identify poly (A) at the 3' end of the HeLa cell messenger and of heterogeneous nuclear RNA, by Nakazato, Kopp and Edmonds (*ibid.*, 1472). Here the terminal sugar is oxidized with periodate, and the resulting glycol reduced with tritiated borohydride, so as to introduce an end-group label. The bulk of this label is recovered in the nuclease-resistant poly (A) fraction. In addition, however, the heterogeneous nuclear RNA appears to contain a short run of poly (A), some twenty residues long, which is not by the same criteria at the 3' end. The presence of the bulk of the poly (A) at

this end of the nuclear RNA supports the view that the messenger comprises the 3' end of the precursor.

CONSERVATION

Sheep or Grouse?

from our Plant Ecology Correspondent
CONSERVATION involves the practical management or manipulation of ecosystems in such a way that a predetermined ecological goal can be achieved. The goal itself is often selected on aesthetic, emotive, or economic grounds rather than scientific ones. A good example of this principle at work is found in the current issue of *Biological Conservation* (5, 41; 1973), in which Watson and O'Hare describe experiments designed to "improve" the Irish blanket bogs.

The experiments which they describe are aimed at increasing the red grouse population of the Donegal and Mayo bogs with a view to improving the shooting potential of the area and hence enhancing tourism. The experiments consist of fencing plots to exclude grazing animals and fertilizing them with superphosphates (502 kg ha⁻¹), potassium sulphate (125 kg ha⁻¹) and smaller quantities of other nutrients. Some plots were also drained. Two years after draining and fertilizing, heather (the staple food of the red grouse) had a productivity four times that in control plots and the phosphorus content of its tissue was twice that of the controls. The density of grouse in the treated areas had also increased by a factor of two and their breeding success was three times that of birds in control areas.

As a management system for increasing the density of grouse in the short term, drainage and fertilization must be considered a most effective treatment. The authors do not, however, stress the fact that these results were obtained in the absence of stock grazing. This is unfortunate, for the work of Watson in Scotland (*Nature Conservancy Research in Scotland, Report for 1968-70*, NERC) demonstrates that without the exclusion of grazing animals, fertilization produces no change in the population density of grouse. The choice, therefore, seems to be sheep or grouse.

There are other ecological questions which should be raised with respect to this work. Watson and O'Hare have found that heather under low phosphate conditions responds well to added phosphate. In natural ecosystems, however, a species which can survive, even at sub-optimal growth, in a poor environment may be at a considerable advantage in avoiding competition from more aggressive species. Raising the nutrient status of a bog ecosystem could destroy this balance and lead to the displacement of heather by more nutrient-demanding, vigorous plants. The long-term effects of drainage and fertilization could be harmful, even to grouse. Eutrophication of many systems has a short-term beneficial influence on productivity which is not maintained.

In western Ireland the economic consideration must be paramount in selecting an appropriate management system. The authors are unwise, however, in suggesting that such a system would also result in improved conditions for wildlife in general. This type of management is likely to benefit only the grouse and the shooting fraternity.

New Case of Disulphide Degeneracy

THE proteolytic enzyme, papain, as conventionally prepared, contains the active enzyme, with a free thiol group at position 25 in the chain, an inactive component, which can be activated by exposure to reducing agents, and a dead fraction in which the vital thiol is thought to be oxidized. By eliminating protective thiols from the preparation, Brocklehurst and Kierstan have isolated a fraction which contains some thiol groups, but is nevertheless inactive: they report their findings in next Wednesday's *Nature New Biology* (April 11).

Whereas the thiol group in active papain is highly reactive towards a disulphide reagent, dipyrildisulphide, even at acid pH, that of the inactive fraction behaves differently, the pH-reaction rate profile corresponding to pK of 7.6. When reduced this material is converted to normal active papain.

Reaction of the thiol groups with radioactive isodoacetamide, followed by reduction, showed that two different types of thiol groups, in respect of pH-reactivity dependence, are present in the activated species. This can be interpreted to indicate that in the thiol-containing inactive species, cys-25 is involved in a disulphide bond with one of the other six half-cystines in the molecule, and that conversion to the active enzyme ensues by thiol-disulphide exchange.

A detailed activation scheme involving thiol-disulphide exchange between the active site cys-25 and residues at positions 22 and 63 is suggested. Two other proteins with the rare property of containing both cystine and cystine residues, have previously been shown to exist in structurally degenerate states, in which alternative disulphide pairings occur.

MAMMAL-LIKE REPTILES

Lines of Evolution

from our Vertebrate
Palaeontology Correspondent

THE lineage of the mammals, through the mammal-like reptiles or synapsids, has been distinct from that of other reptiles for as long as reptiles are known to have existed—since the Lower Pennsylvanian, more than 300 million years ago. Reisz (*Bull. Mus. Comp. Zool. Harv.*, **144**, 27; 1972) has recently supported the view that *Protoclepsydraps*, from the Lower Pennsylvanian of Nova Scotia, is probably a synapsid. He also describes a variety of synapsids from the Middle Pennsylvanian of the same area, which show that the radiation of the group had already begun by that time. One of them, *Archaeothyris*, is very similar to the Lower Pennsylvanian primitive reptile *Hylonomus*, differing chiefly in possessing the synapsid temporal opening. Reisz notes that this opening permits a more efficient use of the jaw musculature, allowing the jaws to be opened more widely and closed more rapidly. He suggests that this was an important factor in allowing the increased size that characterized the Permian synapsids, and enabled the group to become the dominant reptiles of that age.

Study of the remaining mammal-like reptiles has long been hindered by two factors. The first is the lack of adequate links between the Lower Permian forms and their Upper Permian descendants. This void was to some extent filled by the discovery of a Russian fauna, intermediate in both structure and age. Proper understanding of their relationships with the Upper Permian synapsids, however, has still been slow to appear, because of the second difficulty. Ironically, this has been the superabundance of material in the Upper Permian Karroo fauna of South Africa. Presented with an exhaustible supply of new material, chiefly skulls, such earlier workers as Broom produced a stream of reports in which the external cranial features were used as the basis for the erection of new taxa, often using characters which today seem trivial and unreliable. To describe the internal anatomy of the skull, or the postcranial skeleton, would have necessitated more time-consuming mechanical methods for preparing the material.

Only recently have palaeontologists at last begun to reduce the vastly inflated list of South African genera and species of mammal-like reptiles, and more refined methods of preparation are also making it possible to study in more detail such aspects as cranial mechanics. To take as an example the gorgonopsians—carnivorous forms with large stabbing teeth—Sigogneau (*Cahier Pal.*; CNRS, Paris, 417 pp; 1970) has

reduced the original sixty-seven genera to a mere twenty-six valid genera, plus eleven genera of uncertain position (most of which are not even determinable as gorgonopsian!). The functional anatomy of the Gorgonopsia, especially their jaw mechanics, has been studied by Kemp (*Phil. Trans. Roy. Soc.*, **B256**, 1; 1969). He concludes that the group is so distinct from the other advanced synapsids (the therocephalians and the cynodonts) that it should be raised to the rank of a suborder, the other two groups remaining in the suborder Theriodonta. Sigogneau's work accepts earlier views that a few of the gorgonopsians belong in a separate group, the ictidorhinids. In a joint article, Sigogneau and Tchudinov (*Palaeovertebrata*, **5**, 79; 1972) suggest that these two types of gorgonopsians may have

evolved from two separate groups of the known Russian earlier mammal-like reptiles.

In a more recent report, Kemp (*Phil. Trans. Roy. Soc.*, **B264**, 1; 1972) supports the view of some earlier workers that the cynodonts evolved from the therocephalians. He believes, however, that the therocephalian ancestor was an early whaitsiid—a rather late group which is usually regarded as highly specialized. Kemp's theory is not without its difficulties, but it does provide a new approach to the problem of cynodont ancestry.

The cynodonts themselves are of particular interest, because it is from this group of mammal-like reptiles that the mammals evolved. Their classification has recently been revised by Hopson and Kitching (*Palaeont. Afric.*, **14**, 71;

Variations in Variscan Granites

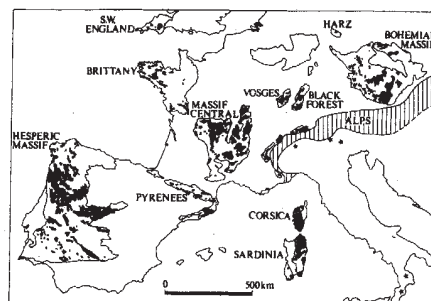
In two recent articles, Hall (*Contr. Mineral. Petrol.*, **32**, 186; 1971 and *Mineral. Mag.*, **38**, 847; 1972) has suggested that the differences in the average compositions of the granites in the Variscan, Caledonian and Alpine orogenic belts, and the compositional variations within the Caledonian belt, may both be attributed to variations in the geothermal gradient. In *Nature Physical Science* this week (April 2), he takes these studies a stage further by showing that the correlation within the belt between composition and geothermal gradient also applies to the Variscan granites.

The compositional part of the correlation is represented by trend surfaces (up to the sixth degree) for normative quartz and albite, based on chemical analyses of all those granites with reasonably certain Variscan ages and taking account of the post-Variscan rotations of Iberia, Corsica and Sardinia. In the case of normative quartz, the trend surfaces for all six degrees are highly significant statistically (>95 per cent confidence level) and so the sixth degree surface, which exhibits regional variations down to the 100 km scale, was chosen for the actual correlation. For normative albite, confidence levels fall off rapidly from the fourth degree onwards and so the third degree surface (>99 per cent) was used for correlation purposes.

The corresponding Variscan geothermal gradients have been assessed on the basis of the relative degree of metamorphism, low and high pressure metamorphism being representative of high and low geothermal gradients, respectively. Low pressure metamorphism of Variscan age is present in most of the granites shown on the map, although convincing evidence of Variscan high pressure metamorphism is found only in the Hesperic Massif. The

granites of the Hesperic Massif are, however, also the poorest in normative quartz and the richest in normative albite—in other words, these extremes correlate with the evidence for low geothermal gradients. The sense of this correlation is the same as that for the Caledonian orogenic belt.

Finally, Hall goes on to show that the Hesperic Massif in albite (rich and poor in quartz) is also the area where the granite magma developed under the highest water pressures. This fact ties in with his interpretation of the correlation between composition and geothermal gradient, which is that the various magmas formed by melting in the crust at a depth governed by the geothermal gradient. For a normal temperature gradient of about 30° C per km, the required temperature for a granitic melt at the base of the crust would only be reached if the melt were saturated with water. But for a higher geothermal gradient, either undersaturated melting would be possible at the base of the crust or melting at a lower degree of undersaturation would be possible higher up in the crust. Thus in general the relatively high geothermal gradient corresponds to a magma with a relatively low water pressure, and vice-versa.



The distribution of granites (black areas) in the Variscan massifs (shown in outline) of western Europe.

1972), who include both the herbivorous tritylodonts and the carnivorous tri-theodonts ("ictidosaur") as end members of the group. Within the cynodonts, mammalian ancestry seems to have lain within the Brazilian family Chiniquodontidae, and Romer (*Breviora*, **344**, 1; 1970) has described the skull of a genus, *Probainognathus*, which may lie on the direct line of mammalian evolution. The postcranial skeleton of cynodonts, and the evolution of that of mammals, has recently been thoroughly described by Jenkins (*Bull. Peabody Mus. Nat. Hist.*, **36**, 1; 1971).

Though much work remains to be done, particularly in elucidating the relationship between the theriocephalian and cynodont stock and the Russian earlier forms, the principal lines of evolution of the mammal-like reptiles, and the adaptive reasons for their structural divergences, are now becoming steadily clearer.

PSYCHOLINGUISTICS

Learning Syntax

from our Experimental Psychology Correspondent

THE view of language acquisition that one derives from Chomsky's emphasis on syntax is that of a child acquiring a working theory of grammar, by being equipped initially with a set of hypotheses between which he has to choose on the basis of the rather small set of utterances he hears. In this sense, as Chomsky points out, the child's task is not logically different from that of the linguist constructing a theory of grammar. A quite different view would be that children do not discover the syntactic rules of a language by finding the underlying regularities of strings of sounds, but that they use their understanding of the semantic setting in which utterances are made to understand words and the significance of their grammatical relationships.

This second view seems a good deal more plausible, but there is the difficulty of describing exactly what kinds of semantic arrangement give rise to grammatical rules. It is in this context that two recent articles by Moeser and Bregman (*J. Verb. Learn. Verb. Behav.*, **11**, 759; 1972; and **12**, 91; 1973) are of interest. In their experiments Moeser and Bregman taught first college students, and then school-age children a miniature artificial language, which had a simple underlying grammar consisting of a small set of rewrite rules. The language was taught by exposing subjects successively to well formed sentences in that language, or by exposing them to this same set of sentences plus pictures of possible

referents for the language. The words in the language were nonsense syllables, and the syntactic rewrite rules were of the form $S \rightarrow AP + BP$; $AP \rightarrow (D) + A$ and so on: meaning that a sentence S can be rewritten as an A phrase followed by a B phrase; an A phrase is a D followed by an A and so on. After exposure to each block of eighty well-formed examples, subjects were asked to make judgments of which of a pair of test sentences (without pictures) were grammatically correct, where neither of the test sentences had previously been seen.

In their first experiment Moeser and Bregman compared performance among four groups of subjects shown (1) words only, (2) words plus a row of arbitrary pictorial figures, with each one always corresponding uniquely to a word, (3) words plus a row of figures each displaying a property referred to by the word, and (4) composite figures using these same reference classes as (3), but made up in such a way that the logical constraints of the pictures were mirrored by the syntax rules. Thus a two-word A phrase YOW FET would have as the pictorial referent in its (3) condition a dotted line and, separately, a green rectangle, but in its (4) condition it would have these reference classes combined to form a green rectangle bounded by dotted lines. In their first experiment they found increasing ease of learning the syntactic rules across the conditions (1) to (4).

In their second experiment Moeser and Bregman gave much longer practice with a total of 3,200 presentations of well-formed sentences, and compared simply the equivalent of conditions (1) and (4) in six subjects who were somewhat younger (aged 12 to 18) than those of the first experiment. Here the results were much more clear cut. Whereas the three subjects trained only with words performed at or near chance with the pairs of test sentences, those trained with words and semantic referents approached completely correct judgments on the grammaticality of the test sentences. The subjects who had successfully learned the syntactic rules with the help of semantic referents were furthermore able to learn the syntactic categories of new words each presented in just three novel sentences without pictures. Subjects trained in the words-only condition did not learn the syntactic categories of the new words.

This sort of experiment does not, of course, show that syntax is learned by appreciating the semantic constraints of referents, but it does show that grammar can be learned much more easily when this is possible. It would be surprising if this were not made use of in a child's initial acquisition of language.

MARS

Up Hill Down Dale

from our Soviet Correspondent

PHOTOMETRIC data from the Mars 2 and Mars 3 probes have provided new estimates of the heights of surface features on the planet. The method is based on the measurement of reflected solar radiation in the CO_2 bands at 2.06 μm and 2.01 μm , and in the continuous spectrum around 2.25 μm . The pressure of CO_2 on the surface is determined from the intensity of the bands, and hence the height of the surface at that point may be calculated from a barometric formula.

The method has already been used in observations from Earth, but a resolution of only 500 to 1,000 km was obtained. Results of this kind for individual areas of the Martian surface, with considerably better resolution, were obtained by Mariners 6 and 7. The Mars 2 and 3 experiments were intended to give a continuous scan of the planet's surface using a field of vision of 0.01 rad (15 km from a distance of 1,500 km), although some averaging of results was carried out before data transmission so that each averaged reading corresponded to some 50 km of flight of the spacecraft.

Reporting their data (*Doklady Akad. Nauk SSSR*, **208**, 1048; 1973), the team from the Institute of Space Research of the Soviet Academy of Sciences stress that the results are "preliminary".

Working with the 6 mbar isobar as an arbitrary baseline, they quote data for the Mars 3 trajectory of February 16, 1972, which passed over the northern extremity of Hellespontus, the north-west edge of Hellas, the dark regions Iapigia and Syrtis Major, and also Meroe and Umbra and the north polar cap. They report a maximum height for Syrtis Major (+3.5 km) and minimum heights for Hellas (-1 km) and the region to the north of Syrtis Major (0 to +1 km). North of 50° N the presence of the polar cap introduces considerable anomalies in the equivalent band width of CO_2 . Comparison of these data with those of the similar trajectory of December 27, 1971, shows that on the earlier date, during the dust storm, a considerable decrease in the band width was observed, confirming earlier observations from Earth made by one of the present team (Moroz) and by Parkinson and Hunten (*Science*, **175**, 323; 1972).

The Mars 3 trajectory of February 28, 1972, passed over Nereidum Fretum, Mare Erythraeum, Margaritifer Sinus, Chryse and Mare Acidaliu, and also gave an anomalous reading for the north polar cap. In the equatorial zone, altitudes were considerably less than those recorded on February 16, 1972,

which confirms radar and spectroscopy measurements from Earth. The greatest difference between the readings for the two tracks was approximately 6 km. The Soviet data for the region of latitude 40° S differed considerably from the Mariner 6 ultraviolet readings, which gave heights some 3 km lower. This may have been the result of a local ultraviolet haze during the Mariner 6 readings.

LAKES AND SEAS

Surface Circulation

from a Correspondent

IN a recent issue of the *Proceedings of the National Academy of Sciences* (70, 93; 1973) Emery and Csanady report their investigations of the surface circulation of forty lakes, marginal seas, estuaries and lagoons, all in the Northern Hemisphere. Their studies are based on records obtained by various means, such as surface drifters, current meters used from anchored ships, drogues, buoyed fishing nets, and theoretical calculations based on the temperature and salinity stratification. Few, if any, of these results were based on their own measurements. They find that with one exception, that of the Aral Sea (45° 00' N, 60° 30' E) in the Soviet Union, the circulation is always anticlockwise if the mean is taken over a sufficiently long time. The examples include all the Great Lakes, Lake Constance, Lake Geneva, the Caspian Sea and the Dead Sea, and the marginal seas include the Baltic Sea, the Black Sea, the Adriatic Sea, Hudson Bay and the Persian Gulf. Unfortunately no records were obtainable for the Southern Hemisphere, but the implication is that the circulation there is clockwise.

Emery and Csanady discuss various mechanisms by which such a circulation could be set up. They discount the effect of long surface waves and internal waves, even though these would be propagated in an anticlockwise direction, for their effects would average out over the times considered. They ascribe the effect instead to Ekman drift. This follows from Ekman's theory of wind-driven currents, which was propounded in the early 1900s and allows for the effect of the Coriolis force. This arises because in establishing and using equations of motion based on a set of axes fixed on Earth rather than in space there seems to be a force acting towards the right of the direction of movement and proportional to the speed when there is motion with respect to these axes. Ekman showed that when the wind acts on water for a long time, this force has to be counterbalanced by a movement of water to the right of the wind direction. This movement is called the Ekman drift. It is then suggested

that when a temperature-stratified water basin is heated while under the action of the wind, there is, looking along the wind direction, a movement of warm surface water from left to right and the water on the left is replenished by colder water ascending from underneath. In the end the right side of the basin has warmer surface water than the left.

Emery and Csanady then go on to suggest, further, that the drag of the wind on the water surface depends strongly on the difference between the temperature of the air and of the surface water. When the air temperature is less than that of the water, the air close to the water surface tends to become warmer than the air above and ascends. This leads to the formation of eddies and turbulence which increase the frictional effect of the wind. Thus under certain conditions the wind drag on the right of the water basin would be greater than on the left and so there would be a net anticlockwise circulation.

This theory is quite a plausible one. It is, however, possible to argue against the view that the effect of internal waves can be neglected; I have recorded such waves with a period of 48 h in a relatively small lake and there may well be some with even longer periods in the basins considered by Emery and Csanady. The amplitude of these waves would depend on the stratification and this could change significantly during the wave period and so lead to asymmetry and a net current transport. The validity of this view depends on the length of time for which the records have been taken, and this is not stated. The anomalous behaviour of the Aral Sea may be a consequence of some such effect.

SEDIMENTOLOGY

Tectonic Control

from our Structural Geology Correspondent

IT is seldom that cross-fertilization between different scientific disciplines is as successful as that demonstrated by Friend and Moody-Stuart (*Skr. Norsk Polarinst.*, 157; 1972) in their intricate analysis of Devonian palaeogeography in Spitsbergen. They chose for their analysis the Wood Bay Formation, the most extensive formation in the 8 km thick late orogenic Silurian and Devonian red bed succession in Spitsbergen. The formation is 3 km thick, outcrops over an area 150 km by 75 km, and represents deposition in river channels and clay flats (playa lakes). They collected field data relating to river palaeocurrents, the sandstone composition and the variation in grain size and plotted contours for these variables by means of a computer-based iterative-fit quadratic trend surface analysis. The results of this initial analysis showed that, in the east, rivers flowed into the alluvial area from the south-east carrying a high proportion of bed-load cross-stratified sands characteristically containing orthoclase feldspar, whereas in the west the rivers flowed from the west depositing a significant proportion of suspended-load sediment characterized by flat-laminated siltstones.

Having reached this stage in the analysis of the alluvial areas Friend and Moody-Stuart attempted to extend the palaeogeography by reconstructing the situation in the source areas. They used laboratory studies of water flow and knowledge of present rivers to interpret the characteristics of the Wood Bay

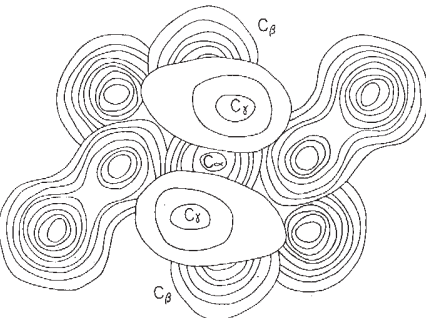
Structure of 1,1-Cyclopentenedicarboxylic Acid

IN an article in next Monday's *Nature Physical Science* (April 9) Margulis *et al.* report how many physicochemical techniques—X-ray diffraction and several kinds of magnetic resonance—have been brought to bear on a study of the problem of molecular motion in the solid state. They have examined crystals of 1,1-cyclopentenedicarboxylic

acid ($C_5H_6(COOH)_2$) and also the cyclopentyl-1-carboxylic acid radical and have come to the conclusion that at room temperature the radical undergoes "pseudorotation". The five-membered ring of the molecule also turns out to be disordered, probably as a consequence of pseudorotation.

The basic ring of cyclopentane, C_5H_{10} , has been known for many years to consist of two puckered formations which have about the same energy, about 5 kcalorie mol^{-1} smaller than that of the planar formation. And it is also a well established idea that the puckering can be thought of in terms of a rotation of the phase of the puckering around the ring, "pseudorotation".

The electron density map produced by Margulis *et al.* of 1,1-cyclopentenedicarboxylic acid, viewed along the molecular and crystallographic two-fold axis, is shown in the diagram.



ivers in terms of the dependent variables of channel type, depth, flow strength and slope.

The profiles of rivers existing today are known to be strongly related to the nature of the sediment load that the rivers carry. Rivers carrying a bed load have braided, almost straight, channels and are some sixty times as wide as they are deep, whereas rivers carrying a suspended load or a mixed load have channels which meander and have a smaller ratio of width to depth (about ten). The depth of a river channel is given by the thickness of an individual graded sand cycle which is deposited by the river in a single unit during flood conditions. Thus Friend and Moody-Stuart were able to recognize two distinct river systems. An eastern one was characterized by bed-load braided channels, averaging, at the high stage, at least 2 to 3 m in depth, 120 to 170 m in width, and low sinuosity. The regional slope was gentle (15 cm km^{-1}) and towards the north-north-west. The western system was a complete contrast, having channels with flood depths of about 0.7 m of mixed or suspended-load type. They were highly sinuous, and about 7 m wide, depositing sediment on a relatively steep slope (100 cm km^{-1}), which faced east-north-east, and extending from a source only a short distance away.

From these values pertinent to the two river systems the authors calculated the mean annual discharge from each and hence the relative size of the source areas; the eastern source area turned out to be about 400 times greater than the western one. From this information they plotted the source areas and obtained approximate limits on the relief from a knowledge of the total volume of the Wood Bay Formation derived by denudation from these source areas. Thus the eastern area occupied an area of some $10,000 \text{ km}^2$ extending some 200 km south-east of the present alluvial area and with a relief of $\sim 6,000 \text{ m}$, whereas the much smaller western source area was $\sim 25 \text{ km}^2$ with a relief of $\sim 150 \text{ m}$.

The full significance of this reconstruction is not apparent until one realizes that the obvious source area for the Wood Bay Formation is the denuded Caledonian mountain system of Ny Friesland, situated immediately to the east of the Wood Bay Formation outcrop and separated from it by the north-south Balliolbreen Fault. These mountains have had at least 10 km of Heckla Hoek sediments and metasediments eroded from them, and orthoclase and heavy mineral detritus within the Wood Bay Formation is similar to that in the Heckla Hoek. Friend and Moody-Stuart suggest that sinistral late Devonian (Svalbardian) movement along the Balliolbreen Fault carried

Ny Friesland from a source position 200 km south-south-east of the Wood Bay Formation to its present position. This is in accord with earlier suggestions by Harland (*Phil. Trans. Roy. Soc. Lond.*, **258**; 1965) that late Devonian sinistral transcurrent movement affected the whole North Atlantic area.

PHYSICAL METALLURGY

EM in extremis

from our Materials Science Correspondent

IN 1938 two crystallographers, Guinier in Paris and Preston in London, independently found smudges on their X-ray Laue photographs prepared from age-hardened duralumin crystals, and independently postulated that the smudges were caused by thin plates, or "zones", of copper trying to precipitate out of solution in aluminium. (*Nature*, as so often, was in at the birth (**142**, 569, 570; 1938).) Thereupon Preston moved on to other interests (see his obituary in *Nature* recently (**238**, 362; 1972)) and it was left to Guinier to spend many years of painstaking X-ray diffraction piecing together the structural details of the formations which have ever since been known as Guinier-Preston zones—GP zones for short. X-ray diffraction, however beautifully used, is a somewhat inadequate instrument when one has to analyse a population of zones which differ in all their dimensions and even in their fine structure, and so the advent of thin-film electron microscopy in the 1950s brought about a revival of interest in GP zones: the hope was to observe them directly.

The difficulty was that the zones in their early stages were known to be ultra-thin, possibly only one atomic diameter thick, and this was quite beyond the resolving power of the

microscopes of 10 to 15 years ago. The zones are, however, surrounded by a field of elastic strain: this field is both the source of the hardening associated with the presence of the zones and the means by which it is possible to see the zones in an electron microscope. The strained aluminium lattice diffracts electrons either more or less efficiently than the unstrained lattice, and thus the photographs image the strain fields rather than the zones themselves—the smile instead of the Cheshire cat. The trouble was that a picture of the smile failed to carry any information about the Cheshire cat's teeth.

Now at last it is possible to see the zones themselves and thus to measure their length and thickness accurately. Two recent papers (Phillips, *Acta Metallurgica*, **21**, 219; 1973; and Phillips and Tanner, *ibid.*, **21**, 441; 1973) reproduce electron micrographs of GP zones at unprecedented resolution. The first report is concerned with Al-Cu, the second with a closely related alloy, Cu-Be. In each instance, (002) lattice planes of the matrix are imaged as sharp fringes at a spacing of about 2 Å; at an early stage of ageing, GP zones of monatomic thickness are revealed as disturbances of these fringes. In Cu-Be, the zones are shown up by local brightness reversal of the lattice fringes, whereas in Al-Cu, Phillips actually succeeded in demonstrating a localized reduction in lattice spacing near the zones, the direct expression of the associated strain field. This is a remarkable technical achievement in electron microscopy, and opens the way to a more exact interpretation of the early stages of age hardening; a start has already been made by using the observations to identify the zone dimensions corresponding to the maximum in electrical resistivity during the ageing of a Cu-Be alloy.

Radio Emission from Nebulae Around Stars

IN next Monday's *Nature Physical Science* (April 9) Hjellming, Blankenship and Balick report the detection of radio emission associated with the stars MWC 349 and RY Scuti. This confirms earlier observations of the sources at several radiofrequencies, and provides evidence that the radio emission comes from resolvable radio "nebularities" with thermal radio spectra.

The observations now reported were made at 2,695 and 8,085 MHz with the NRAO interferometer. Neither source showed any signs of variability at these frequencies during the period of observation (October 1972 to February 1973 for MWC 349; February 19 to 28, 1973 for RY Scuti), both have the same kind of visibility function, and both were nearly unresolved at 2,695 MHz with the longest interferometer spacing

(2,700 m); this suggests sizes $\lesssim 3 \text{ arc s}$. There is also an indication of complex structure in both sources.

So it seems that the radio emission is definitely not coming from a point source in either case. The emission comes instead from compact regions surrounding each star, and Hjellming *et al.* point out that it would be incorrect to call these objects "radio stars" because they do not show the strong variability, indicating "an intimate relationship" with the underlying star, that the name has come to imply. These two sources have, in fact, some similarities to planetary nebulae with electron temperatures around 10^4 K , dominant emission measures of $\sim 10^8 \text{ pc cm}^{-6}$, and in general similar properties to such well-known planetary nebulae as NGC 7027.

CAREERS FOR SCIENTISTS

Science Graduates Must Widen their Horizons

THE articles which appear on the following pages are designed to give students who are graduating this summer some assistance in choosing a career. This is the time of year when, traditionally, undergraduates are feverishly trying to decide between the merits of one employer and another, but this year, as in the past few years, there are fewer employers on the horizon and those that are in the market want many fewer new graduate employees than they did in the halcyon days of the early and mid-1960s.

But is the situation now improving? There is some evidence that this year's crop of graduates will find it easier to find a job than their predecessors did last year but it is clear that the prospects are not better in all areas. The message to job seekers is that the traditional avenues of employment are still constricted but that there are reasonable prospects outside the areas where scientists have tended to look for work in the past.

The British graduate had little difficulty in selling his skills until the late 1960s but he now finds that a degree is no passport to employment. The most difficult year for the new graduate was 1970-71 when the British manufacturing industries took on only 6,500 new graduates compared with the almost 8,000 who accepted employment in these sectors in 1968-69 and 1969-70. Although the figures for 1971-72 are not yet available it seems unlikely that they will differ significantly from those obtained for 1970-71, but the cheering note is that the manufacturing industries this year are expected to recruit to a greater extent than they have done in the past two years. The numbers will, however, still be lower than for 1968-70.

Most arts graduates have, since time immemorial, progressed from university to employment in which they have not directly used the knowledge which they acquired as undergraduates. But the scientist after graduating has always expected to apply his knowledge in a more direct fashion. But now this traditional type of employment for the scientists is becoming increasingly less available and in the long term it is probable that this is not wholly a bad thing both for the graduate and his employer. Whatever employment the graduate scientist enters into, and in recent years this has included banking, sales and accountancy which have always been the preserves of the arts graduates, he does not stop making use of his science. He is therefore most certainly not a lapsed scientist, for whatever such a person is he is not someone with a degree in science who is earning his living without directly applying the results of his training.

University careers officers have realized during the past two years that the job patterns for science graduates are changing and the slightly better prospects which this year's graduates face are a direct consequence in no small measure of the efforts which have been made by the universities to find alternative employment for the science graduates outside these traditional areas. Employers, who in the past saw no need for science graduates but who have recently taken scientists on to their staffs, should now be more open to suggestions

that they should employ more scientists—presumably in jobs which were until a few years ago the prerogative of arts graduates.

In recent years the numbers of science graduates entering employment have decreased and, not surprisingly, there has been an increase in the numbers taking courses in teacher training after graduating to compensate for this. But the numbers continuing in academic study have remained substantially constant since 1969. One of the most startling statistics to emerge from the articles on the following pages, however, is that between 1970 and 1971 the percentage of first degree mathematicians and statisticians who took up scientific work, which does not include teaching, decreased from almost 32 per cent to 17.7 per cent (see page 380). Physicists also suffered a similar decrease in demand from 29.1 per cent in 1970 to 19.5 per cent in 1971 but biologists and medical students have suffered no such large change, for in 1971 19 per cent of that year's first degree graduates took up scientific work compared with 22.3 per cent in 1969 and 18.9 per cent in 1970. The higher degree graduates fared slightly better but mathematicians and statisticians were still not needed to carry out scientific work to the same extent in 1971 as they were in 1970.

Even if job prospects for scientists in science-based industries are not getting substantially better, there are at least no large scale predictions being made now that the situation must first get worse before it improves. But the job situation in the science-based industries is expected to improve although there is no one in the know who is prepared to be bold enough to set a time on this. It is, however, universally agreed that the heady days for science graduates which occurred in the early and mid-1960s will not return. But, that said, there is universal optimism that the present slump is only temporary. Scientists in the United States have also suffered the same setbacks, but in 1972 the chemical industry in that country broke all records and the industry is starting to move upwards again. The British chemical industry is expected to follow suit soon although it will, quite properly, take time to readjust afterwards before it begins to think of increasing its intake of new graduates. The long term prospects for the scientist in industry are therefore not altogether bleak.

EDUCATION AND CAREER OF THE SCIENCE GRADUATE	376
TENTATIVE PROGNOSIS OF FUTURE DEMAND	378
TRENDS IN EMPLOYMENT OF GRADUATE SCIENTISTS	380
PLANNING FOR CHANGED EXPECTATIONS	381
EMPLOYMENT OF GRADUATES IN PHYSICS	383
PROSPECTS FOR GRADUATING CHEMISTS IN 1973	384
CAREERS FOR BIOLOGY GRADUATES	387
EMPLOYMENT OF SCIENCE GRADUATES IN AUSTRALIA	388

Education and Career of the Science Graduate

F. S. DAINTON

Department of Physical Chemistry, University of Oxford

The changing role of the science graduate will present a particular challenge to the university teacher, who will also have to come to terms with the somewhat changed conditions under which he may in future do his own research.

I WRITE this article in Japan. This statement is not a bizarre irrelevancy, for Japan illustrates dramatically how rapidly human attitudes can change and human energies be redirected. On August 15, 1945, the people of Japan heard with awe the Emperor's words of acceptance of the terms of surrender imposed by the Allies. Not many Europeans will have noticed that Emperor Hirohito concluded his statement with an exhortation to the Japanese people to "Unite your total strength . . . so that you may . . . keep pace with the progress of the world". On the time scale of human history Japanese ethos and values changed overnight from the militaristic to the pacific, and now Japan has transformed herself from a semi-feudal to a democratic state in which in 1972 the Director General of Higher Education and Science in its Ministry of Education could justifiably maintain "that in higher education in Japan, equal opportunity of education is offered to all boys and girls without distinction of the social standing and occupational status of the families they come from".

In this state with twice the population of Britain there are four times as many students in degree level courses as in Britain, and rather more than twice as many new graduates in science, engineering and technology are launched on to the labour market each year. Moreover, there is no doubt in the minds of the Japanese that this rapid progress towards mass higher education should be maintained. The stark contrast between this optimistic attitude, doubtless partly attributable to the buoyant economy, and our present cautious approach, which is partly associated with the seemingly diminished employment prospects of our graduates in science and engineering, prompts the following home thoughts from abroad.

A Few Facts

Table 1 illustrates the growth of higher education in Britain during the past decade and a half. Within the rapidly growing university sector, virtually all the entrants to which have at least two A-levels and many have three A-levels, the number of science-based students has, despite the swing away from science, been maintained at a slightly higher level than that

of non-science-based students, and the output of graduates has been in the same ratio. Disregarding Japanese university students who are studying subjects not normally found in the curriculum of British universities, for example home economics, teaching and nursing, only about one third of Japanese students have been studying science-based subjects. Moreover, in the 1960s Britain doubled its output of graduate scientists, excluding medicine and allied fields, from all forms of higher education to reach about 26,000 in 1970, whereas the corresponding figure for Japan is 76,000. In Britain, however, these graduates are almost equally divided between "pure" and "applied" science, whereas, even allowing for the fact that many courses of study which are in engineering faculties in Japan would be brigaded under pure science faculties in Britain, the Japanese pure science graduates form a far smaller fraction of the total.

From 1945 to 1969 all reasonably competent new graduates in science, engineering and technology in Britain found it easy to secure congenial and adequately remunerative employment, chiefly either in industry or in higher education. The proportions entering each of these spheres of employment did, of course, differ from subject to subject and also depended on the level and quality of the qualifications held. For the most part engineers and technologists entered industry. Of the pure scientists, chemists were most oriented towards industry, so by 1971 66% of all chemists were employed in industry, 20% in schools or higher education and only 10% in local or national government or in research institutes; earth scientists, physicists and biologists, on the other hand, became decreasingly industry-oriented during this period. In the 1960s some British scientists went to the United States, constituting the "Brain Drain", which was, however, more the result of the seriously unsatisfied demand in the United States than of an over-supply in Britain. Industry must therefore be thought of, historically at least, as the prime employer of qualified scientists and engineers (QSEs), and in the English-speaking world by the end of the 1960s industrial demand for scientists and engineers who wanted to continue specialist work, at least initially, became less than the supply. Actual unemployment and even dismissal of scientists emerged as a new post-war phenomenon in the United States (but not in Japan) and many commentators began to talk and write about "over-production" of scientists.

Some recent British science graduates have undoubtedly suffered disappointment and some are, perhaps, enduring real hardship. There must be many young people now in the BSc, MSc, or PhD pipelines who are wondering whether they made the right decision at eighteen and twenty-one. They are not reassured by the Cassandra-like utterances of those modern windvanes of real or imaginary change, the university appointments boards officers, especially those from north of the River Trent. All concerned with this problem, whether they be employers, government, educational policy makers, university and polytechnic teachers or, last but not least, sixth formers themselves, need to give careful thought to the issues involved.

Demand for QSEs in Industry

The first question is clearly whether this phenomenon is temporary or is likely to persist. If it is the former, then there

Table 1 Percentage of Eighteen-year-olds Entering Full-time Higher Education

	Universities	Colleges of education	Advanced further education	Total
1955	3.2	2.0	0.5	5.7
1960	4.0	2.7	1.4	8.1
1967	6.0	4.8	3.2	14.0
1971	8.7	5.7	4.8	19.2

is little ground for change in our educational patterns except for that which rests on the need to improve for its own sake the educational experience which young people have in British higher education. I suspect that this diagnosis is incorrect. The signs are that industry will not wish to recruit highly trained but narrow specialists in the numbers likely to emerge from an unchanged higher educational system. Growth of research rather than development is unlikely to continue at the rate of the past two decades if only because many industries are made increasingly wary of innovation by the increasingly high costs and risks involved in development and marketing. Moreover, within the developed world national social priorities are changing, and as we move into the post-industrial society the growth of manufacturing industry may give place to greater growth in the service industries, reflecting desires for conservation of energy and raw materials, improvement of the quality of life and similar objectives. The science graduate will therefore have to enlarge his employment horizons. Many specialists will still be needed, but increasingly they will be working within multidisciplinary groups where their ability to communicate will be at a premium. The roles of individuals may also be expected to change more frequently; even in the 1960s it was the case that by the age of forty more than half the QSEs were engaged on work for which the detailed factual knowledge of first or second degree was thought by them to be of little consequence, either because it was outdated or because it was unnecessary. In my experience industrial QSEs, whether engaged in specialist scientific work or in management, technical service, sales, planning or any other of the multifarious duties they may be called on to perform, value much more highly the skills of logical analysis and an ability to apply these to new situations and of cogent verbal presentation of arguments to colleagues often less well equipped technically, together with more personal qualities of being able to cooperate fruitfully with others, often from different original disciplines, and the ability to gain the reciprocal cooperation of these colleagues.

University Lecturers

What of the employment prospects in the education sector? In the short term one must look to the White Paper on education¹ for guidance. The provision of nursery schools, the raising of the school leaving age, and the filling of vacancies for science teachers will provide employment opportunities for only a minute fraction of science graduates at the bachelor level. Institutions in higher education will, of course, continue to need able people at the PhD and higher levels, but in view of the slightly declining portion of science students in universities, the displacement downwards of staff/student ratios, and the often far from buoyant numbers of would-be science entrants to polytechnics, it is idle to pretend that recruitment in this sector will in the next decade approach the high levels of the mid-1960s.

It is also proper to ask whether the aspirations about university life of many would-be lecturers are likely to be fulfilled to the same degree as in the recent past. Many will wonder about their chances of unfettered choice of research and of securing the necessary financial resources. The government has stated in its White Paper on research and development² (Command 5046, para. 54) that it "attaches great importance to the support which all the research councils give to the universities and the new arrangements are designed to ensure that this should continue unimpeded". This statement must be taken literally, but "unimpeded" should not be read as "unchanged". The research councils are rightly considering, in the light of the quinquennial settlement, how best to assist in maintaining university research at the highest international level. Recent policies concerning selectivity will be reviewed and priorities will change; the role of postgraduate students may well be rescrutinized and attempts will have to be made to discover what validity attaches to the oft-repeated view that

involvement of a teacher in original research is essential if his teaching is not to be "stale, flat and unprofitable". But by and large the British university lecturer may in the 1970s expect his opportunities to do worthwhile research to be undiminished, even though he may increasingly do it cooperatively with colleagues from the same or other institutions and make use of national or international facilities and of trained research assistants rather than of so many research students, and he must be prepared to respond to mild external stimuli from the research councils or from government departments. As John Stuart Mill might have said, "His liberty will be the privilege of self-discipline".

If, however, his research role is to be little affected, the lecturer and his colleagues should expect to do some hard rethinking about their teaching. In view of the changed career expectancy of science graduates to which I have already referred, he will need to ponder how many of his students he should subject to a curriculum designed, as it is at present, primarily to produce a competent specialist researcher and what new educational single or multiple-subject programmes he should devise and encourage his other students to take.

Hortatory Comments

The twenty thousand or so scientists, engineers, medicos and so on on the staff of British universities are an unusually able group and exert a critical influence on the intellectual and personal development of the more gifted of our young people. Although the rate of recruitment of new staff will not match that of the 1960s, those appointed may look forward to an interesting and rewarding career in research and teaching, but collectively they will need to be more innovative than their predecessors because the career prospects of their students will be more diverse. In this connexion I would conclude with two comments which I believe to be of considerable importance. First, the lecturer should not allow his enthusiasm for his subject to diminish and he should aim to transmit that enthusiasm to his students. But in his teaching he should also remember that the most useful things he can give to his students are those habits of thought to which I have referred and which will be of permanent value in the students' subsequent career. He should impart to his students the conviction that the experimental sciences can be an excellent preparation for many careers, for they involve a unique blend of numeracy and literacy, of appreciation of the importance of accurate observation, of induction and deduction, of model building and model testing, of logic and of judgment which all of us, whatever our careers, have cause to use repeatedly. It follows that if science graduates can match in conversability and communicability their arts contemporaries it is the latter who will have to fear the competition of the scientists as these increasingly penetrate the traditional employment preserves of the arts graduates. The university lecturers will therefore have to think hard about the learning process of their own students and how to modify the curriculum content and pedagogy to improve the chances of his student acquiring or strengthening these desirable qualities.

Second, if our society is to be successful, both it and its individual members must be flexible and able to adapt rapidly to new opportunities and situations. This will in turn increase the number of reorientations which graduates will need to make in their careers. They will need help in their retraining and the universities and polytechnics are the obvious means of providing this help. But to be effective they will need to draw on the knowledge of industry and on resources which are additional to those which the University Grants Committee is now able to provide and university staff should be giving urgent attention to the best ways of carrying out this task.

¹ *Education: A Framework for Expansion*, Cmnd 5174 (HMSO, 1972).

² *Framework for Government Research and Development*, Cmnd 5046, para. 54 (HMSO, 1972).

Tentative Prognosis of Future Demand

B. J. HOLLOWAY

University of Manchester Careers and Appointments Service, 333 Oxford Road, Manchester M13 9PU

Higher education in science should never be specifically for employment, but if it can retain its educative function, while taking some cognizance of the possible shape of the market for its products, the frustrations and disappointments suffered by so many individual science graduates during the past few years may be somewhat lessened in future.

THE abrupt fall in the demand for QSEs (the accepted abbreviation for "qualified scientists, technologists and engineers") which occurred in Britain at about the beginning of 1971 is now a matter of history, and statistics are available which indicate its severity. The number of QSEs declared redundant during the past two or three years can only be guessed at, but that other sensitive indicator of demand, the total recruitment of science, technology and engineering graduates at BSc, MSc, or PhD levels to British manufacturing industry for first employment after graduation, is known to have been about 8,000 in both 1968-69 and 1969-70, but in 1970-71 it fell sharply to 6,500. If one omits the relatively stable categories of building, contracting, civil engineering, public utilities, the National Coal Board (NCB) and the United Kingdom Atomic Energy Authority (UKAEA), whose demands for graduates were fairly steady over the three years, then for oil, chemical, engineering and "other manufacturing" industries the drop was from 6,300 to 4,650—a fall of more than 25%. Figures for 1971-72, relating to the position at December 31, 1972, of those who graduated between October 1, 1971, and September 30, 1972, are not yet available, but indications are that they will not be significantly higher than those for 1970-71. When the first occupational destinations of 1973 graduates are collected by university careers and appointments services in the autumn and winter of 1973-74, it seems likely that these will show a recovery in recruiting figures to a point perhaps half-way back to the level reached in 1968-70. Within this more hopeful overall picture, however, there are still black spots in chemical industry (except pharmaceuticals), oil and mechanical engineering, and in heavy electrical industry where the demand for power engineers remains very low indeed. The computer industry, after an even more abrupt "stop", seems to be returning to the "go" phase as an employer of fresh graduates, although at a much lower level than in earlier boom years. Demand is strongest from light electrical industry, including applied physics, with GEC's requirement, for example, increased from about 400 graduates in 1972 to 700 this year.

All this is simple enough stuff, but of practically no use except to 1973 graduates wanting to know something about

their personal prospects in the job hunt now in progress. Looking ahead even to 1974 is difficult, and from 1975 onwards, as Blaug has recently suggested, the process of manpower demand forecasting becomes so chancy as to be intellectually disreputable. But 1975 graduates are now first-year students in universities committed to their subjects, 1976 graduates have already made their choice in the form of applications submitted to the University Central Council on Admissions (UCCA) and the first generation of uncommitted students cannot therefore graduate before 1977 or reach what now seems to be accepted as full professional level as a working scientist or technologist (a PhD degree or something like three years of other relevant postgraduate training or experience) before 1980. If these latter students ask now about their employment prospects they must be given an answer of some sort, which itself must be based on a view of the longer term trends. This, for what it is worth, is one man's view of these trends.

Partial Recovery

It seems now to be agreed that, for manufacturing industry at least, the heady days of the early to mid-1960s graduate recruitment programmes are unlikely to return. Demand will recover, but to a lower level. Economies of scale have been operating in capital-intensive and graduate-intensive industries like chemicals, oil, electric power, steel and computers, and future recruitment levels in these industries will relate more to the replacement than to the augmentation of technical staff. For example, the overall effect of spending £3,000 million in the steel industry will be minus 50,000 jobs, and it is expected that organizations such as Imperial Chemical Industries (ICI) and Shell, whose contribution to growth is still large, will employ fewer staff of all kinds in the 1980s than they do today, even after the streamlining and heavy redundancies of the past two or three years. Within the overall reduction of 10 to 20% in complements the proportion of graduates may rise, but programmes of job enrichment, job enlargement and management development will continue to destroy jobs at all levels, not only those at the lower levels of skill. So, too, while inflation lasts, will the need continue for control of wage and salary costs.

Perhaps most significant for the scientist or technologist is the death in recent years of the assumption that more and better research and development lead almost inevitably to growth, national or institutional. It has been destroyed by the spectacular bankruptcy of Rolls-Royce, the demolition of the "spin off" argument as an economic justification for research and the relative rundown, partly at least on sheer cost grounds, of aerospace programmes in the United States and elsewhere. Defence programmes will no doubt get more expensive, but will not need proportionately more qualified staff.

All of these elements in the analysis of the supply and demand position are long term rather than short term in nature.

Even when, for example, the chemical industry re-enters a phase of high investment, perhaps two or three years hence, it would be naive to expect the creation of substantial numbers of extra jobs for chemists or chemical engineers. To attribute the problems of the past two years primarily to lack of economic growth is to be absurdly over-optimistic.

Other Employers

Manufacturing industry is far from being the only employer of QSEs. In 1968, according to the Department of Trade and Industry, there were in all nearly 350,000 economically active QSEs (185,000 technologists and engineers and 162,000 scientists) of whom 43% of the engineers and only 25% of the scientists were employed in manufacturing industry. Education was a larger employer of scientists (36%) with government and research (11%) being the only other significant employer except for "other" at 10%. Large numbers of engineers were employed in public utilities (11%), local authorities and construction (12%) and government and research (8%), with only 7% in education. It looks very doubtful whether government and research as an employer of scientists can be expected to increase its demand in the next few years, for neither the Scientific Civil Service nor the UKAEA is exactly a growth area, and "public utilities and construction" may be very hard put to it to absorb its quota of the large number of potential professional engineers already in the pipeline. Recent calculations by Head¹ about civil engineers suggest particular difficulties there in what has hitherto been a firm market. The ability of higher education to absorb its own products is on the wane, with a heavy age bias of existing staffs towards the lower end and a limited expansion of university and polytechnic science and technology departments envisaged in the White Paper². Only something like 1 to 2% of today's university science graduates can expect to enter university lecturing jobs, and the equivalent departments in polytechnics and other institutions of higher education can expand no faster than the supply of students studying science in the sixth forms of secondary schools. Even that apparently insatiable market, science teaching in schools, is already visibly tightening and, apart from local pockets which may remain, the phrase "shortage of science teachers" will pass out of the language within two or three years. Headmasters and headmistresses advertising their vacancies can already testify to the startling change in sentiment in this market.

Although the longer-term effects of Britain in Europe, in terms of QSEs, are hard to forecast, it seems clear that in the developed part of the English-speaking world, with the minor exception of a small and temporarily unsatisfied market in South Africa, the same conditions of apparent oversupply exist; and in the developing countries the attitude towards expatriates and towards western science and technology has changed so much in recent years that no substantial market for the export of British QSEs exists there. No resurgence of the brain drain can be expected.

So prospects for the science graduate wishing to be employed as a scientist look relatively poor, statistically speaking, during the next 5 to 7 years. He can hope for a slow recovery of traditional demand, but my own view, expressed at a conference in 1971, that "this is the first of the seven lean years" was more optimistic than that of Professor R. V. Jones at the same conference, who thought that it would be the late 1980s to early 1990s before specific demand had again fully caught up with supply.

Where Will They Go?

Two mechanisms are available to mop up "surplus" science and technology graduates who are not employable in their professional contexts. The first is the rapid takeover by graduates of technician-level jobs, a transition which has been

in progress since the early 1960s. For example, statistics show an increase of 11,700 QSEs employed in manufacturing industry in 1968 by comparison with 1965, but they also show an increase of 11,750 in the number of QSEs employed as "technicians and technical supporting staff"—a very remarkable 1:1 ratio. But such a takeover provokes some pointed questions about the best method of educating and training technicians—namely whether this should be by university or CNA degree course, by the old-style HNC or by today's HND, full-time or sandwich. It is by no means certain that employers would vote in favour of the university degree for this purpose.

The second mechanism is by the entry of QSEs into first occupations in which their specific knowledge and training are of little, or even of no, direct significance. This is not necessarily a matter for regret. *The Long Term Demand for Scientific Manpower*³, published in 1961, had this to say on the subject. "In our view the possibility that there will be a surplus of scientists over immediate demands for employment should be welcomed. It should make possible a rational, as opposed to an emergency, use of the scientific disciplines. It should mean that at long last we shall have a supply of qualified manpower with a scientific training for management, administration, and the professions generally, in addition to those who up to the present have been drawn inevitably into vocational employment. We do not doubt that scientific education will adjust itself to this new prospect; and that in the same way as only a proportion of those trained in the classics and history have expected to find employment in their own fields of study, an increasing proportion of those trained in specialized scientific disciplines will obtain employment outside them. We think that both science and the nation will benefit from this adjustment."

This opportunity for "both science and the nation" is now with us as a continuing situation. No longer need the science graduate feel that he ought not to seek quite different employment, for example, in social work, accountancy, librarianship, non-scientific government service, banking or the retail trade; the skills he has acquired can now be looked at by him and by potential employers in a much less circumscribed manner. Even academic staff are slowly taking this point. And his skills may prove an unexpectedly marketable commodity; for example, the number of science and engineering graduates entering chartered accountancy articles today is increasing fast, but apparently at the expense of pure arts graduates, fewer of whom are now entering this occupation. But the very size of this movement of graduates away from first employment of "relevant" type—with, for example, up to 40% of chemistry graduates (and according to one source something like 70% of chemical engineering graduates) currently entering "non-relevant" jobs—makes it necessary to re-examine the nature and assumptions of scientific education at universities. Pippard's proposals some years ago of a framework of very general science courses for two years, as a preliminary to specialized study of a single subject at the equivalent of honours degree level, made great sense then but makes even greater sense now. If the BSc (or the Diploma in Higher Education, DipHE) could be taken in two years, and could include a liberal scattering of non-science courses, only those wanting and expecting to be employed later in their scientific discipline—perhaps 50% of the total entry—would specifically need to go on to the next two years of specialized study. The others could be offered the alternative of further broad courses, but not of a lower level, as a prelude to non-scientific employment. Under such a system choices of course having some significance for a future career could be made at a later age than is the case at present, and closer to the point in time at which employment decisions can no longer be postponed.

¹ Head C. R., *Surveyor*, CXLI (January 12 and 19, 1973).

² *Education: A Framework for Expansion*, Cmnd 5174 (HMSO, 1972).

³ *The Long Term Demand for Scientific Manpower*, Cmnd 1490 (HMSO, 1961).

Trends in Employment of Graduate Scientists

W. P. KIRKMAN & D. F. T. WINTER

Cambridge University Appointments Board, 6 Chaucer Road, Cambridge CB2 2EE

In the past few years the most striking change has been the reduction in the number of first degree graduates entering employment and the increase in the number opting for teacher training.

If graduate employment statistics had been collected in the Middle Ages, they would doubtless have shown that theologians went largely into jobs where their theological studies were directly relevant. More generally, the relationship between subject of study and job would have been direct and predictable.

Few arts graduates today would expect to "use their subject" on leaving a university, unless they had decided to teach. Until recently, most scientists could assume that they would be able to continue to work in jobs in which their scientific knowledge and training were directly relevant.

For the past two or three years, however, that has not been the case. It ceased to be true for chemists rather earlier than for physicists, and there were other differences in the speed of the change between the different scientific disciplines, but as a generalization scientists are now in a position little different from that which has long faced arts graduates. They may enter jobs in which their subject is of practical, professional value, but the likelihood is that they will not.

Tables 1 and 2 present the picture, and indicate how great

some of the changes have been, particularly for the first degree science graduates. The total numbers entering employment on graduating have fallen, whereas the numbers embarking on teacher training have risen. Even more significant are the changes in the types of work to which those entering employment have gone. Research and development, scientific support and services to management, general training and line management, all show substantial reductions. Financial work has attracted many more scientists, and so has social work (though the total numbers are small).

The fact that biologists and chemists have been facing difficulties if they wished to work in their subject has been well publicized but, as Table 1 shows, the fall in the proportion of those taking specifically scientific work (other than teaching) is even more startling in the case of the mathematicians and the physicists.

Table 2, relating to higher degree graduates, shows a different balance, but essentially tells the same story. Research and development, with fewer openings, has drawn more on those with postgraduate qualifications than on first degree graduates. Even so, more higher degree graduates entering industry have taken up line management, rather than purely scientific, jobs.

These figures reflect sharp reductions in demand from the euphoric 1960s, as Holloway suggests in his article on page 378 of this issue. To say that there are fewer "science" jobs, and therefore that more scientists are going, and will go, into jobs where their science is a general educational rather than a specifically vocational qualification, though true, is not, how-

Table 1 Statistics of British First Degree Science Graduates

	1969	1970	1971
First destination			
Academic study home	3,390	3,557	3,436
Academic study overseas	188	130	95
Teacher training	1,848	1,914	2,279
Social work training	24	17	21
Other training	688	767	741
Employment home permanent	5,150	4,866	4,405
Employment home temporary			392
Employment overseas	388	411	352
Other categories	1,868	2,198	2,324
Total	13,544	13,860	14,045
Type of work			
Scientific research and development	1,184	979	768
Scientific support and services to management	1,893	1,756	1,289
General training and line management	804	818	651
Financial work	206	270	605
Social work	26	36	81
Teaching and lecturing	331	273	338
Professional training	589	539	489
Other categories	217	195	184
Total	5,150	4,866	4,405
Proportion taking scientific work outside teaching	%	%	%
Biology and medicine	22.3	18.9	19.0
Chemistry	24.3	22.2	15.1
Mathematics and statistics	35.5	31.9	17.7
Geology and environmental science	14.2	17.3	15.1
Physics	31.4	29.1	19.5
Combined courses	22.3	20.6	14.2
All scientists	26.4	23.7	17.4

Table 2 Statistics of British Higher Degree Science Graduates

	1969	1970	1971
First destination			
Academic study home	618	666	734
Academic study overseas	394	326	254
Teacher training	32	31	50
Social work training			3
Other training	16	21	34
Employment home permanent	1,451	1,509	1,583
Employment home temporary			47
Employment overseas	310	332	299
Other categories	1,309	1,470	1,648
Total	4,130	4,355	4,652
Type of work			
Scientific research and development	725	753	756
Scientific support and services to management	204	250	235
General training and line management	53	56	126
Financial work	5	15	15
Social work	2	3	7
Teaching and lecturing	425	394	396
Professional training	5	12	25
Other categories	32	26	23
Total	1,451	1,509	1,583
Proportion taking scientific work outside teaching	%	%	%
Biology and medicine	17.0	17.1	19.8
Chemistry	27.4	29.2	24.9
Mathematics and statistics	22.1	28.9	18.5
Geology and environmental science	18.8	17.9	16.9
Physics	23.3	23.4	22.5
Combined courses	30.0	18.7	14.3
All scientists	22.6	23.3	21.0

ever, the whole truth. Other factors must be taken into account.

These include, notably, the changing attitudes of employers and of science graduates themselves, and the nature of employment in an age of rapid technological change.

In the case of attitudes, there is frequently a pendulum effect. From a policy of seeking as many scientists as could be persuaded to apply, industry has tended to turn to a policy of questioning their value to the organization in any numbers. Almost inevitably, such questioning produces exaggeration but it is surely intrinsically healthy. Recruitment should never take place unless a real need has been established and real prospects can be offered.

Graduates themselves similarly show exaggerated reactions, which are perfectly understandable. If industry ceases to woo them, and indeed questions their desirability, they will naturally tend to look for other suitors. Professional qualifications have an obvious attraction, and financial work, for which they may be well suited, still offers opportunities to obtain them.

The changing nature of employment presents rather more fundamental problems. Scientists who decide to become chartered accountants, or marketing men, or librarians, are consciously leaving their science behind them (except as an extremely useful base). Even those who enter employment where their scientific skills have provided the *entrée*, however, must now recognize that the speed of technological development may well render those skills obsolete before they have completed their working life (indeed, probably will do so). They may continue to work as scientists but work will be punctuated by the need to relearn and refresh themselves, to move in directions which they could hardly have foreseen on emerging from the university or polytechnic.

This brings one back to attitudes. Scientists are no longer in the position of the mediaeval theologians. They have moved from that position of happy certainty far more brusquely than the theologians had to do, but for those who can contemplate the new reality as one of hope rather than disaster, the prospect is by no means unpleasing.

Planning for Changed Expectations

D. S. DAVIES

Imperial Chemical Industries Ltd, Millbank, London SW1 3JF

In solving their own employment problems, scientists will ensure that they serve society in new ways.

THERE are now 350,000 qualified scientists and engineers (QSEs) in Britain, alongside a population of 550,000 who have reached a similar level of further education outside science. At present, the annual output of 15,000 scientists and 10,000 engineers is showing no sign of increase, and we seem to be heading for a state of affairs around the years 2000 to 2020 in which there will be a population of about 1 million QSEs and about 4 million other highly educated people in Britain. If science and engineering regain popularity, their proportion of the total will increase. Further education is undoubtedly going to be given to more than 20% of each age group by the early 1980s, so we are moving very rapidly away from a society in which further education was for people who would (as of right) enlighten and command about twenty times as many less educated and, hopefully, admiring contemporaries. Instead, the graduates will now have to do much more of the world's work themselves.

New Model

If one wishes to perpetuate the old "command" model (and I, for one, do not), then of course we are overproducing graduates. There is no possibility of having this number of centuries with no centuries to look after, and avoiding trouble. If, however, one takes the view that a boss-and-minion society can be replaced by something more cooperative and participative, then we are not producing too many highly educated people and can do with more, provided that planning is sensible and that people are told what to expect. Few would accept the observation currently to be seen painted on a shelter on Hampstead Heath: "All exercise of authority perverts: all submission to authority humiliates", but we must

listen with a lot of sympathy to our children's strictures on our structures.

Before tackling the current problem of careers for scientists, one must take a longer look at the coming decades. I am not speaking for industry, or ICI, or the Science Research Council, or even for my generation: I am speaking for myself. We must begin by going beyond the Robbins justification¹ for educating so many people in general and scientists in particular. We only have experience of three kinds of workable economy. The first is an authoritarian and stable system in which most people are forced to accept their lot, as in the ancient civilizations and in mediaeval Europe. The second, which eventually undermines the first, is a reformist, self-help system driven by a desire to escape from poverty and disease, as in nineteenth century England. The third, a natural development of the second, is a largely affluent system driven by a desire to grow in affluence, as is the case in Western nations now. None of these three systems needs mass advanced education, but those who are educated will perceive soonest that neither the second nor the third can continue indefinitely. In due course, overcrowding, aggression, and resource problems become dominant, and produce results that outweigh the advantage of further conventional advance. At this stage, which is certainly in sight, one either has to return to the first, authoritarian, non-growth system (to which the necessary meritocracy will be strongly hostile) or devise a growth system that generates a range of desirable activities which are neither productive of aggression nor demanding of resources other than human energy. Aldous Huxley, in *Brave New World*, described a prime example: a game entitled "centrifugal bumble-puppy". The range could include the writing and reading of poetry or literature, the composing and performance of music, or the painting and contemplation of pictures. If it includes cooking, it will have to be *haute cuisine*, and must not generate obesity. It cannot encompass the performance of services such as sweeping and cleaning, because these activities will inevitably be made unnecessary or improved in productivity (thus employing fewer) in an affluent society.

The factor commonly required for these permissible elements

in the ultimate growth economy is education. One key reason for this is that it will aid acceptance of the idea that population must be stabilized. Even more important, it provides a basis for understanding and demanding more music (performing and hearing) and more poetry (writing as well as reading), all of which can satisfy competitive urges. Sport will play a key part, but success in sport cannot be universal and its utility is therefore self-limiting. Only education can provide unlimited channels for acceptable cupidity, acquisitiveness and avarice. Alongside poetry and music can be set thought-intensive science. To discover pulsars, one needs equipment, but it is necessary to escape from the tyranny of programmed but basically foolish computers. Consequently, a big programme of discovery could involve an army of people obtaining satisfaction from the output of one big dish or one incessantly bleeping satellite telescope assembly: some would be trying out models of cosmic dynamics, and some would be poring over digital or photographic outputs. But computers would be used sparingly and intelligently. Intelligence-intensive biology would take its place alongside this intelligence-intensive cosmology.

The food, clothing, communications apparatus, permitted vehicles, and dwellings for this population would be produced by a small proportion of the work force, whose resource-winning and recycle procedures would be driven by solar photons, fixed in various ways. And a branch of this water-drawing and wood-hewing supertechnological fraternity would be preserving health for as long as mental capability lasted, edging life spans cautiously upwards but not so as to increase the population of mental and geriatric hospitals—and, with success, chipping back birth rates. There would be a strong link between these medical managers and the biologists in the “permitted growth” sector.

With such a pragmatic Utopia (and there really are not many workable alternatives other than much nastier tyrannies accompanied by periodic war and famine), there are plenty of careers for scientists: but chiefly for scientists with a keen perception of the limitations within which they may work. Because of the crucial importance of this acceptance of constraint, scientists will have to be strongly social animals, with broad perceptions.

Short Term Needs and Opportunities

The 1973–80 career question for scientists, and our future educational programmes, can only be looked at sensibly in terms of pictures such as that just painted or better alternatives. Many of the more immediate occupational patterns must be temporary, and one does not want a succession of disappointments, following the current dismay and discomfort arising from the fulfilment of Sir Michael Swann’s largely unheeded prophecies of the mid-1960s². The following points seem to be germane.

- The educational sector will saturate quite soon with scientists needed for teaching specialist science, whether at school or elsewhere. Thereafter, as the science teaching population will be young, wastage by retirement will be small, and the intake of specialist teachers will also be small. But there will be golden opportunities for more science to be included in general education, and for new arts and skills to develop in the vital business of spreading scientific awareness through the whole population.

- The science-based industries, whose costs are rising fast and whose technologies are becoming mature in many (but not all) areas, must improve the productivity of their scientists and managers, as indeed they must improve all productivity. To keep their age structure, costs, and promotion prospects reasonable, they should seek to maintain an intake of new graduates by encouraging some of those in the age range 25 to 35 to move into smaller companies who are their suppliers and customers, taking relevant skills with them. (Engineers

have always done this.) But even with this measure, intake will be only a modest proportion of that during the 1960s. It will be vital to face all these problems, and pay in such a way as to make this sort of career pattern attractive.

- Public sector science will employ more scientists in and alongside administration, in particular in matters concerning the environment and trade. But standards will be high, and numbers only modest.

- The service industries (public and private sector) will increasingly employ scientists who are prepared to make initially dull jobs interesting—ranging from hospital management through waste disposal to transport logistics and economics. All of these subjects have tended to be regarded as humdrum, and to be treated as not specially worthy of good thinking and logic. All have, more recently, been greatly enriched by combinations of operations research and imaginative technology.

- The whole field of first-level management now involves a difficult mixture of the understanding of people, equipment, and systems. The traditional training of the “on the job” foreman is increasingly inadequate, although many foremen impressively succeed in educating themselves informally. Graduates will be needed as supervisors in many new types of plant, and this will account for quite rapidly growing numbers.

- Marketing does not have a glamorous intellectual image; at its dullest, it involves routine visits and, at its least scrupulous, it can involve misrepresentation. Yet assisting people to discover their real needs, and then filling them, is skilled and of vital importance both to the customer and the whole process of innovation. Good profits can only be earned by making products that are appreciably nearer true needs than most other products—whether these discovered needs require new formulations, new chemistry, different engineering, or altered materials. Good graduates, encouraged to use their skills, can transform a market, and larger numbers will almost certainly be so employed.

- The professions (law and accountancy, for example) are receiving a bigger input of science graduates. This too is to be welcomed: a cost accountant who understands science is likely to bring a fresh approach and to be able to help a manager to ask for performance numbers and norms that will concentrate on essentials: more important, he will find it easier to stop collecting and displaying numbers that are useless because of irrelevance or inaccuracy. The proportion of scientists in these classes could become substantial.

The exploration of these new opportunities has to be done chiefly by the job-hunting graduate himself: employers are not usually over-anxious to change their sources of staff without persuasion. Consequently, graduates need help and encouragement to knock on new kinds of door and to know what to say when the door is opened an inch or two. Supervisors can help in this and some special exercises such as the SRC’s one-week courses, principally run by the Careers Research and Advisory Centre (CRAC), can further embolden. Graduates in America have been employed more widely for many years now, and American experience is well worth scrutiny.

Science graduates, then, have to become more closely involved with society for two reasons. The first, and more immediate, is to help provide goods and services more efficiently, thus creating leisure. The second is to help society to use this leisure without using too many resources, making the place unpleasant to live in, or increasing disturbance and neurosis. In solving their own employment problems, scientists will be getting their skills used more fruitfully than ever before.

¹ *Report of the Committee on Higher Education (Robbins Report)*, Cmd 2154 (HMSO, 1962).

² *The Flow into Employment of Scientists, Engineers and Technologists (Swann Report)*, paragraph 32, Cmd 3541 (HMSO, 1968).

Employment of Graduates in Physics

L. COHEN

The Institute of Physics, 47 Belgrave Square, London SW1X 8QX

Although the employment prospects for physicists in industry may improve in some cases in the next few years, medical physics and accountancy, banking and commerce may well be new growth areas.

In considering the present and future employment prospects for graduates in physics it is useful first to establish the demographic background. According to the best official estimates the number of economically active physics graduates increased from about 15,000 in 1959 to more than 30,000 in 1972. If the number of those graduating in physics each year remains constant this "stock" will rise to 40,000 by 1976. A small fall in new enrolments to physics departments will not seriously alter this short-term extrapolation.

The question arising from these figures is simply whether there has been and will be an increase in the number of jobs for physicists to match the increase in the numbers in the market for employment. In attempting to answer this question it is necessary from the outset to differentiate between employment which requires the specialist skills and knowledge of the physicist *pur sang* and employment for which a knowledge of physics is useful but not essential or, alternatively, where the intellectual discipline of the study of physics is a good foundation. One might call these "traditional" and "non-traditional", although the non-traditional type of employment has been significant since physics emerged as a separate university subject.

In the 1950s and most of the 1960s there was considerable expansion in the number of jobs of the traditional type both in industry and in higher education (arising from the growth of colleges of advanced technology and the new universities). By the end of the 1960s university expansion had virtually stopped and British industry was having to cope with great economic problems. It was at this time, some three years ago, that the job market for scientists and engineers changed from a seller's to a buyer's market.

Recent Trends

At this stage it would be useful to see what employment new physics graduates have been obtaining recently. The University Grants Committee (UGC) produces comprehensive statistics on the first employment of graduates but this is not published until the end of the following year, hence the figures for 1971 are the most recent from this source. These show that among first degree graduates about 30% went on to further study, about 15% went into teacher training and about 35% obtained permanent employment. The remainder were overseas graduates returning home, in other training and so on. Of those in permanent employment 18% were in public service (central and local government), 12% in education (at all levels), 50% in industry and 20% in commerce and other non-technical activities. Of higher degree graduates 20% were able to continue with research by means of, for example, post-doctoral fellowships while about 50% went into permanent employment;

of this latter category 10% were in public service, 30% in education (mostly universities), 40% in industry and the balance in commerce, etc.

A recent survey of the 1972 first degree graduates carried out by the Institute of Physics, to which about a third of the graduates replied, revealed that 37% were studying for a higher degree, 16% were undergoing teacher training and about 40% were in permanent employment, mostly in industry or public service. Of those in employment more than a third said they were in jobs which did not require their physics. Among all of those who had obtained a post, that is, employment and further education of all kinds, only a small percentage, however, said that they were not satisfied with their position. Perhaps it was too soon for disillusionment to have set in.

The starting salaries of those in employment, excluding further education and training, showed a considerable range but two-thirds of the salaries were between £1,200 and £1,600 a year. It is not known how job satisfaction correlates with salary.

It is a reasonable assumption that, in the short term at least, those who employ physicists now will continue to do so. In government service and higher education the situation seems to be quite straightforward although it should be remembered that many of the people in departments of engineering, particularly electrical engineering, metallurgy and others, have at least a first degree in physics.

It is, however, in industry that the greatest variety is probably shown. A recent analysis of the firms employing members of the Institute of Physics showed that by far the largest group of employers was the electrical and electronic industry, which accounts for something like 40% of the total. The chemical industry and general engineering were the next largest groups but each had less than 10% of the total. There was a wide spread of employment in other sectors but in all cases the numbers involved were small.

This survey did, of course, cover people who had been in employment for varying lengths of time and would therefore reflect the employment situation as it had been in past years. There is, however, career development with any one individual and it is important to look at this, for a new graduate should be interested not only in what job he is doing in his first years after graduation but what he might be doing ten or twenty years later. It has always been appreciated that a proportion of scientists who have entered industry to carry out research and development move away from this function into technical sales, production, marketing and general management. What was not known was the proportion of those starting in the research and development department (which virtually all scientists did) who moved into other departments and the time scale of the movement.

Changing Expectations

These questions were looked into some four years ago as part of a larger investigation on the relationship between education in physics and the needs of industry. The employment of physicists in twenty or so large companies was looked at in detail and overall it was found that some 40% were employed in non-research and development work. Examining the age distribution showed that for the under 25 age group there

were twice as many in the research and development function as elsewhere. This ratio fell with increasing age until in the 43–50 age group there were more in non-research and development work. It was notable but not unremarkable that those not in research and development were the better paid.

This investigation showed that even five or six years ago the young physics graduate, and his employer, were beginning to realize that it was worthwhile to move out of research and development at an early age rather than to accept this move as an unfortunate necessity when one became too old for research. On the contrary it was found to be a satisfying and rewarding experience. The extension from moving after a year or two in research and development to going straight into non-research activities after graduation was even then apparent.

This transition from "traditional" to "non-traditional" work is well established but it has not usually been a sharp one. In many cases it has been a question of doing a "non-traditional" job in association with active scientists in an industry using science and/or engineering. The time scale of the transition seems to have shortened in recent years but the environment has not been completely unfamiliar, not to say alien, to the physicist.

The new phenomenon is the turning of physicists to "non-traditional" jobs in a "non-traditional" environment. It should be said at once that individual examples of this have occurred for a very long time, most particularly during the economic depression of the late 1920s and early 1930s—a period which has obvious parallels with the present day. The surveys quoted above show that a significant number of new graduates are now going directly into accountancy, banking and commerce, and these numbers will undoubtedly grow in the future.

This movement into the legal and commercial field is by no means restricted to physicists. Figures quoted in the latest annual report of Imperial College showed that between 1965 and 1971 the proportion of all first degree graduates of the college going into these fields increased from 3.1% to 13.1%

and for those with a higher degree from 0.8% to 10.9%. In addition graduates in the arts, humanities and social sciences are also becoming increasingly interested in these fields of employment.

There is much evidence to show that a study of physics forms a good basis for the subsequent study and practice of many other subjects. It is reflected in the fact that more than 90% of those who take physics at A-level do not subsequently specialize in physics, in the fact that many of those with a first degree in physics go on to do a higher degree in another subject and in the fact that many who start out as physicists achieve success in other fields.

Opportunities in the Future

Where does all this lead to in considering the job opportunities and future careers of those who will graduate in physics in the next few years? It seems quite evident that although there will be many "traditional" jobs physicists will have to look increasingly to "non-traditional" areas for employment. In the traditional fields the overall picture does not seem to be one of appreciable expansion, but there may, nevertheless, be limited areas of growth. Medical physics could well be one such area. In industry an improved economic climate will increase the number of jobs but these are more likely to be in production and technical support functions rather than in research and development.

This increasing orientation towards non-traditional employment has to be accepted by the student long before he graduates. It is important that the student's expectations should be realistic and it is now no longer realistic for somebody studying physics to be sure of making a career as a physicist. Of course the high flyers, who are also really dedicated to research or teaching, will continue in traditional paths but the others must accept that their study of physics has been an education, preparing them for many possible careers rather than a vocational training preparing them for only one possible career.

Prospects for Graduating Chemists in 1973

ERIC PARKER

Royal Institute of Chemistry, 30 Russell Square, London WC1B 5DT

The Secretary of the Royal Institute of Chemistry traces the trends in the employment of graduate chemists.

A FEW years ago no one would have been interested in an article of this kind, for it went without saying throughout the 1950s and most of the 1960s that a degree in chemistry was a passport to a secure and interesting job. Indeed the situation for nearly twenty-five years after the end of the Second World War was such that chemistry graduates rarely had to make any effort to find a job because the employers came to them. It was not so in the 1930s and it is not so now; the inevitable question is: are we again in a situation like that of the 1930s? I believe that the answer is clearly no and that the situation today is totally different from what it was between the wars.

In the first place the present situation is a dynamic one— which it was certainly not in the thirties—and as the American sociologist Alvin Toffler has so vividly pointed out in his book *Future Shock*¹ the rate of change and the rate of diversification

in Western society are both increasing with frightening rapidity. Certainly if the number of chemistry graduates in 1971 (the last year for which figures are available) had been the same as it was in 1965—1,731—no one would have had any difficulty in finding a job. In fact, the number of graduates in 1971 was 2,457 and 196 of these were still seeking employment at December 31 in that year (Tables 1 and 2). Table 1 shows that the growth in the number of chemistry graduates has been considerable in the past fifteen years and even though

Table 1 Numbers of First-degree Chemistry Graduates

1958	1,130	1962	1,391	1966	1,967	1970	2,514
1959	1,179	1963	1,431	1967	2,034	1971	2,457
1960	1,418	1964	1,547	1968	2,564		
1961	1,387	1965	1,731	1969	2,679		

The figures do not include CNAA graduates or graduates of the Royal Institute of Chemistry (230 and 358 respectively in 1971) as the patterns of first employment of these graduates are not recorded. It is very unlikely, however, that this exclusion affects the argument developed.

Table 2 Principal Destinations of First-degree Chemistry Graduates as at December 31 of Each Year

Year	Research or academic study (Britain)	Teacher training	Employment in Britain	Seeking permanent employment	Unknown	Total No. of graduates
1968	873	300	1,052	97	87	2,564
1969	877	310	1,061	100	157	2,679
1970	850	319	913	140	145	2,514
1971	764	405	774	196	135	2,457

there has been a decline since 1969 the number graduating in 1971 was still much higher than that in 1958. This, of course, is a result of the principle adopted by the Robbins Committee and followed by successive governments that the number of university places should aim to meet the demand from suitably qualified school-leavers—and not the demand from potential employers. A rate of growth of this magnitude—significantly in excess of the rate of growth of the economy—had of necessity to lead to a situation where supply of graduates exceeded demand and the crunch came for chemists in 1970. It came also in other Western countries at about the same time—or, in some cases, is coming now—and the employment prospects for newly graduated chemists are worse than they are here in, for example, the United States and the Netherlands.

Profits and Employment

The essential reason why the crunch came in 1970 is that this was the year in which large-scale science-based industry, and particularly the chemical industry, started getting into difficulties from world-wide over-capacity and greatly intensified international competition. Seeing its profit margins being rapidly eroded, industry reacted by cutting back on its most expensive raw material, labour, and this included scientists. Although many arguments can be advanced to show that this was a short-sighted policy in terms of the future vitality and innovative capacity of industry it is frankly difficult to see what different decisions anyone could have taken in the circumstances. The result was the mounting unemployment figures, and chemists were no more immune than any other salaried professionals. Indeed, in some ways they were more vulnerable, for their activities do not normally have any direct and immediate effect on profitability.

It is worth examining the figures for industrial employment in some detail, as these provide the key to the whole problem. Table 3 shows that in 1971 only 465 of the 2,457 graduates with first degrees entered private or nationalized industry—about 19%, the proportion having dropped sharply from 31% in 1969. To these figures must be added the number of graduates with higher degrees entering industry; Tables 4 and 5 show that these were 303 and 286 in 1969 and 1971 respectively. (Table 4 also shows, incidentally, that higher-degree graduates in chemistry have not so far been affected by unemployment and this is borne out by surveys of PhD graduates carried out by the Royal Institute of Chemistry for the Committee of Heads of University Chemistry Departments—including a survey of 1972 PhD graduates.) Expressed as a

proportion of those leaving university, either with a first degree or a higher degree, the total number going into industry in Britain dropped from 44% in 1969 to 30% in 1971 (1,129 out of 2,570 in 1969 and 751 out of 2,526 in 1971). This is a substantial drop—and it probably went further in 1972—but it affects less than half of chemistry graduates and it might be thought surprising that it should have such a far-reaching effect. Why, in fact, do the opportunities in the other areas of employment not expand to take up the slack?

The reason, ultimately, is that industry (with commerce) is the only wealth-producing activity in our society and every other activity—education, government research or whatever—must be paid for out of the taxes levied on this created wealth, either by corporate taxation of the company or individual taxation of its employees. The total employment situation is therefore always sensitive to the health of industry and although governments can cushion the effects of an industrial recession for a time they are powerless to do so for long. It is essentially for this reason that government research, which is a traditional area of employment for chemistry graduates, has been cut back during the past few years and is not now in a position to take up any of the slack.

The other traditional area of employment for chemists is teaching and here the intake into schoolteaching, either directly or through teacher training, has shown a marked increase (Tables 2 and 3). There is evidence that this trend continued in 1972, over 10% of PhD graduates entering schoolteaching in that year, and it is very welcome after so many years of inadequate numbers of well-qualified chemistry schoolteachers.

In further and higher education, on the other hand, and particularly in the latter, the expansion in the 1960s was so rapid that we are now inevitably in a period of consolidation and there is very limited opportunity here. Indeed the age structure of chemistry staff in the universities is such that the number of vacancies is likely to be about 4% of the total staff for some years to come—even on the somewhat optimistic assumption that the opportunity is not taken to reduce the establishment of a department when a member of staff leaves.

With the possible exception of schoolteaching, there is then little hope of any early increase in the numbers of vacancies in the areas of employment that are the traditional alternatives to industry for chemistry graduates. What of industry itself? Here the position is brighter. The output of the United States chemical industry broke all records in 1972 and that year has been described as one of the most spectacular in the industry's history. There is little doubt that the chemical industry in Britain has also "bottomed out" and is starting to move up again, and the same is true of other large science-based indus-

Table 3 Principal Categories of First Employment in Britain of First-degree Chemistry Graduates

Year	Public service	Teaching	Oil, chemical and allied industries	Engineering and allied industries	Other manufacturing industries	Nationalized industries and the UKAEA	Accountancy, banking, insurance and other commerce	Total No. going into British employment
1968	75	63	453	140	175	58	51	1,052
1969	66	65	451	154	167	54	65	1,061
1970	53	60	389	119	124	56	74	913
1971	98	88	234	58	117	56	95	774

Table 4 Principal Destinations of Higher-degree Chemistry Graduates as at December 31 of Each Year

Year	Research or academic study (Britain)	Postdoctoral fellowships	Overseas students returned home	Employment in Britain	Seeking permanent employment	Unknown	Total No. of graduates
1968	91	304	107	410	16	67	1,159
1969	93	270	104	463	14	73	1,235
1970	100	230	128	472	15	97	1,247
1971	88	219	108	463	19	122	1,245

tries. Although the upturn is more obvious in terms of the financial health of companies than in any immediate increase in recruiting, the latter must follow soon. The general trend in all employment in Britain is strongly upward and this also must influence the position for chemists, for the outstanding characteristic of changes in employment level is that they produce a snowball effect, either upwards or downwards.

The only real doubt is whether the upturn will come in time to help the 1973 graduates and this must be a matter of speculation. Perhaps all that can be said is that the situation for chemistry graduates in 1973 will be no worse than in 1972 and could well be significantly better; and there will almost certainly be a substantial improvement in 1974 and 1975, especially as the number of chemistry graduates will continue to fall in this period (the number admitted into chemistry courses having fallen every year from 3,308 in 1967 to 2,526 in 1972).

Given that the employment prospects for chemistry graduates in industry will shortly begin to improve—if they have not already done so—there remains the question of what types of employment there will be. There is a growing consensus of view that there will be no return to the situation where large numbers of chemists are recruited into the research and development function, and this has perhaps been put most clearly by Duncan Davies in his 1972 address to the Royal Society on discontinuities in chemistry and chemical technology. He sees the growth areas for employment of chemists in industry as outside the traditional research and development function and this view is supported by a preliminary statistical model for the chemical industry prepared by the Department of Trade and Industry. The model, which is admittedly based on very simple assumptions, predicts for the period 1973–81 that the density of professionally qualified scientists and engineers (QSEs) in research and development will not increase and therefore that the absolute numbers will increase only if there is an increase in total employment in the industry; that the number of QSEs in associate professional (that is, technician) roles will increase at the same rate as between 1965 and 1968; and that the density of QSEs in professional functions outside research and development (production, commerce, and common services including top management) will increase at the same rate as between 1965 and 1968.

Whether these predictions are quantitatively accurate or not, there can be little doubt of their qualitative correctness. It has been officially estimated that by the year 2000 the number of graduates produced annually in all disciplines will be twice

as great as the number of what would now be regarded as “graduate jobs”. Diversification of employment for graduates must therefore take place more and more, and this will require greater adaptability by graduates in subjects like chemistry, which have hitherto been regarded as vocational, than by graduates in arts or social sciences. Furthermore, the diversification will not be confined to industrial employment. Already there is a significant increase in the number of chemistry graduates entering commerce (from 51 in 1968 to 95 in 1971; Table 3) and this trend will certainly continue. Many other areas of potential employment for graduate chemists spring to mind and notable among these is the administrative Civil Service. The point has already been made that the adequacy of government funds depends on the success of industry and commerce in creating the necessary wealth. But the allocation of priorities for government spending is a political decision and political decisions are strongly dependent on advice from senior civil servants. The more scientists there are in such positions therefore, the more likely is it that science and technology will receive their fair share of public resources. This is even more important at the present time, when the image of science has taken a beating in practically all Western countries.

To quote Duncan Davies, “It is possible and correct to meet the new situation with a series of campaigns to secure the employment of many of those trained in science in activities for which technological capability is highly desirable, but where it has hitherto been absent”. He believes that chemistry is “a subject that uses concepts and numbers in even balance . . . and that additionally has much central relevance, so that there is a good argument for according it a place as a central enabling subject, alongside languages and mathematics”.

Here then is a challenge for any chemistry graduate worth his salt. There will continue to be the traditional jobs in research and development and in teaching, but these will form a decreasing proportion of the total; at the same time there will be a growing number of other jobs in a wide variety of different fields and many of these will provide exciting opportunities. As with all jobs, they will ultimately be what the chemist makes of them.

The figures in the tables are taken from the annual University Grants Committee publication *First Employment of University Graduates*, and I acknowledge permission to reproduce them.

¹ Toffler, A., *Future Shock* (Bodley Head, 1970).

Table 5 Principal Categories of First Employment in Britain of Higher-degree Chemistry Graduates

Year	Public service	Teaching	Oil, chemical and allied industries	Engineering and allied industries	Other manufacturing industries	Nationalized industries and the UKAEA	Accountancy, banking, insurance and other commerce	Total No. going into British employment
1968	34	132	149	15	40	24	2	410
1969	32	103	217	30	40	16	7	463
1970	14	110	237	35	45	16	4	472
1971	41	115	197	33	41	15	7	463

Careers for Biology Graduates

D. J. B. COPP

Institute of Biology, 41 Queen's Gate, London SW7 5HU

The Secretary of the Institute of Biology suggests that environmental work, not necessarily of a strictly biological nature, will attract more British biology graduates in the years to come.

BEFORE 1939 the great majority of biologists were teachers, chiefly in schools, with only a few centres outside the universities, such as Kew Gardens and the Natural History Museum, employing research workers. The Second World War, however, changed the pattern: biologists were deployed in pest control, malaria eradication, penicillin production, the development of radar and operational research. After the war the growth in the numbers graduating in biological subjects kept pace with the demands from industry, the research councils and the expanding universities, and there was an approximate balance between demand and supply until about 1970. Throughout the twenty post-war years the only sector in which there was a serious shortage of biologists was school teaching.

By 1970 the output of biologists from universities was three times that of 1960; the numbers entering various types of employment each year from 1959 to 1971 are shown by Fig. 1. The most recent information about the numbers of qualified biologists in various types of employment is given by the

10% sample census made by the Registrar General in 1966 (see Table 1). (Unfortunately the figures from the 1971 census are not yet available.)

Because those considering careers in biology either for themselves or their pupils will have recent or current experience of the situation in schools, there is less need to describe the work of teachers even though those in schools are the largest single group of biologists. At A-level the move to replace the separate subjects of botany and zoology by biology is likely to continue and, if accompanied by a growth in physical science as an A-level subject, could provide well balanced courses for both intending scientists and those wanting a broader education. At lower levels there will be less emphasis on "biology" as a subject with the moves toward integrated science or environmental studies. Hence those attracted to teaching will have opportunities for innovation. The existence of Biology Teachers' Centres and the activities

Table 1 Employment of Biologists in 1966

University teachers	2,185
School and college teachers	6,940
Research work	3,250
Administration/management	940
Technician/industrial worker	1,210
Other	1,695
Total	16,175

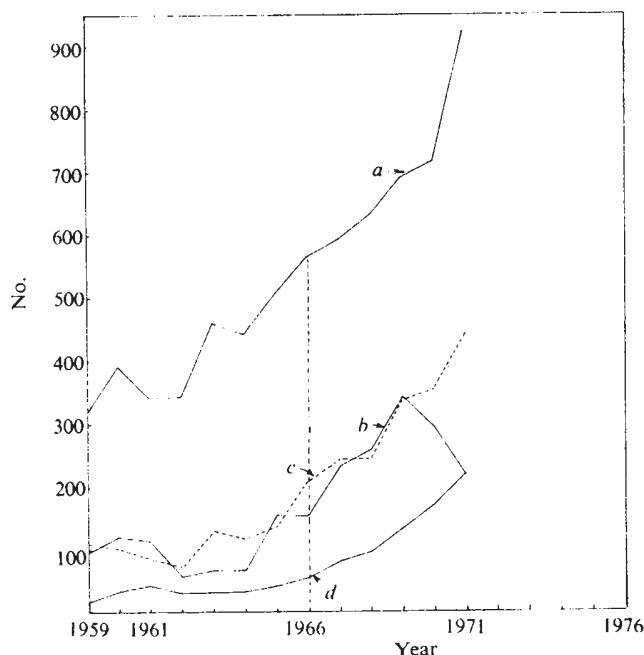


Fig. 1 First employment of biologists with first degrees or equivalent qualifications. *a*, Education; *b*, manufacturing; *c*, government; *d*, other sectors.

of the local branches of the Institute of Biology should enable teachers to overcome any feelings of isolation and help them to keep up to date in their chosen subject.

Posts in the Civil Service and research councils will be available for those who have higher degrees or a good first degree, but recruitment will be principally for replacement, not expansion. When the reorganization of support for government research and development has been completed, which may well not be until the spring of 1974, there could be a number of vacancies for work on new projects. There should be vacancies in the National Health Service when the recommendations by the Zuckerman Committee for a unified scientific service are implemented. Although the largest group of biologists in the NHS will remain that of biochemists, the numbers of microbiologists, immunologists, physiologists and geneticists are growing.

Recruitment by industrial firms has been low recently but there are signs of economic recovery. If this occurs then the food, pharmaceutical and agricultural industries will once again be seeking biologists for research, development and administration. Several companies are developing biological processes for the production of synthetic protein; another possible development is the large-scale extraction of protein from leaves; a few companies already have biologists working on the production of conventional vegetables by tissue culture.

The increasing awareness of the environmental problems arising from the growth of the human population and its

increasing demands for energy and resources ought to lead to the employment of more biologists in the fields of reproductive physiology and biochemistry and on the effects of pollution on living systems. The establishment of Regional Water Authorities in England and Wales will lead to a greater use of chemical and biological knowledge in the management of water resources, and the Royal Commission on Environmental Pollution is at present considering the training desirable for those who will be employed in environmental work at various levels. More biology graduates could well enter the fields of landscape architecture, planning and resource management: they should be prepared to take specialist training so as to become fully qualified in the appropriate profession.

If the numbers entering biological courses at universities and polytechnics remain at the present levels the total population of those with a degree in a biological subject will double

in the next ten years. There is no possibility that the number of posts in teaching and research will grow in proportion and thus many of those obtaining a biological degree must be prepared to consider it as an education but not necessarily a training for specific work in the field of biology. Because they will have become accustomed to dealing with such things as complex systems, variability, the interrelations of organisms and behaviour, biology graduates should be well equipped for posts in management and administration. Those with the level of numeracy now developed by many modern courses in biology will be suited for work in systems analysis and operational research. Those who do not go into research or teaching but into the administrative civil service, information work, industrial management or local government should find satisfaction in work which taxes their intellect even if it does not draw directly on the detailed knowledge acquired in a university course.

Employment of Science Graduates in Australia

S. J. RAWLING & C. G. PLOWMAN

The Australian National University, PO Box 4, Canberra, ACT 2600

The Careers and Appointments Officer and the Academic Registrar of the Australian National University discuss the problems faced by the science graduate in Australia. One of the difficulties is that comprehensive statistics have only recently become available.

AUSTRALIA has a small population and a correspondingly small university enrolment, so one might reasonably expect that a clear picture, statistical and otherwise, could easily be drawn of the output and the employment of science graduates. Unfortunately this is not the case. Even in terms of measuring output, it is difficult to give a precise figure, because of differences of nomenclature and course organization in the universities. It is clear, however, that of the 15,000 bachelor degrees conferred by Australian universities in 1971, approximately 3,000 were in pure or applied science (including such areas as agricultural science, but excluding specifically professional qualifications such as veterinary science).

We can be rather more precise about the output of PhD graduates, for the fields of study are usually more clearly defined. Of the 750 PhD degrees awarded in 1972, approximately 450 were in science and in applied science¹.

Outputs at both the first degree and the PhD levels increased spectacularly during the 1960s. In 1961 the total of all bachelor degrees conferred in Australia was less than 5,000 (15,000 in 1971) and the total number of PhD degrees conferred rose from 174 in 1963 to 750 at present. Particularly in the case of the PhD this increase resulted in large part from a conscious effort to increase both the quantity and the quality of post-graduate training in Australia. The PhD is a relatively new degree, the first having been awarded in 1948, and when the Murray committee² looked at Australian universities in 1957 it saw a great need to provide highly qualified manpower to staff the universities in the predictable boom of the 1960s. The Murray committee, and others in the early 1960s, were

confident that the new outpouring of highly qualified people in the sciences could be absorbed³.

By the mid-1960s, thoughtful observers had begun to question the capacity of Australia's economy and workforce to absorb the large number of science graduates, amongst others, particularly as the needs of tertiary education itself began to be met^{4,5}. At the beginning of the 1970s there is in Australia a graduate employment problem which affects science graduates, although it is not apparently as serious as that which exists in some other countries.

Employment of First-degree Science Graduates

1972 was the first year in which Australia-wide statistics were collected of the destinations of first-degree graduates. Table 1 provides some relevant information from this collection.

We do not have comprehensive figures for previous years which can provide a basis for comparison, although this will be the case in the future. Earlier surveys have been carried out, particularly by appointments services in individual universities, and there have also been special studies of particular subject areas⁷⁻¹². The available data provide something of a picture of the employment of science graduates, at least as far as their first destinations are concerned, but there is little information about their later careers.

The figures for 1972, given in Table 1, do enable us to make concrete certain generalizations which have been aired frequently. Perhaps most important is the fact that a very small percentage of first-degree graduates in science find their way, initially at least, into private industry. Some further comment about employer attitudes will be made in the discussion on PhD graduates. It is also possible to suggest that the traditional first degree in a pure science discipline has not produced graduates who are particularly interested in, or well trained for, industrial applications of science.

Some other general impressions have to remain just that, although they would be agreed on by most people in the appointments field. Many science graduates are finding, and will continue to find, great difficulty in locating a first job

Table 1 Destinations of Selected First-degree Science Graduates in Australia, 1972

First destination	Chief field of study				
	Physical sciences %	Bio-logical sciences %	Earth and environmental sciences* %	Agri-cultural science and forestry %	Mathe-matics† %
Higher degree studies	28	28	16	13	14
Teaching and teacher training	34	29	46	15	39
Other further study	7	9	4	4	9
Public service and other government employment	9	13	11	43	15
Private industry and commerce	10	4	12	8	10
University employment	7	7	3	3	5
Other	5	10	8	14	8

Based on data gathered for the Graduate Careers Council of Australia and appearing in ref. 6. The information relates only to degrees granted by universities. Applied science is excluded because the original tabulation amalgamated applied science and engineering graduates.

* Includes graduates in geography.

† Includes graduates from faculties of arts and so on with mathematics as the principal subject studied.

which is "in science", and this is usually their preference. As the notion of the professional scientist comes almost exclusively to mean someone with a PhD, many BSc graduates will be forced out of the job market in specialist fields, particularly research, and must see themselves as non-specialists. The teaching profession will perhaps begin to find its needs for science teachers met, after years of inadequate supply.

There are changes taking place, affecting first-degree science training, which have a crucial importance for employment. There are moves towards inter-disciplinary courses, one example being a human studies programme at the Australian National University. There is an increasing number of specialist postgraduate courses, whether for diplomas or master's degrees, which can provide a vocational capstone to a first-degree education in science. It is possible now to do such courses in education, librarianship, computing, administration and environmental science. It is very likely that, for many BSc graduates to fit happily into the workforce outside academic and research establishments, an even wider variety of such courses, with industrial applications, needs to be provided. Already, as Table 1 indicates, most BSc graduates feel the need to supplement their first degrees with some sort of further training. It is clear that the BSc is not a professional degree nor a job training.

Another significant change is the increased availability of courses in the applied sciences, both within the universities themselves and in the rapidly expanding colleges of advanced education. The existence of these courses, both in the traditional technological fields and in newer areas such as in environmental science, will have an important bearing on the employment of pure science graduates who stop at the BSc stage. If it is obvious that only a minority of those now undertaking pure science degrees will be able to work in pure science, the question must be asked whether a training in some applied area will be more appropriate for many. The role of the first degree in pure science other than as preparation for research careers should perhaps be the subject of healthy questioning. An examination of the factors relating to the employment of first-degree science graduates done with the same depth and thoroughness as the Academy of Science study of PhD graduates mentioned below would be most useful.

Employment of PhDs

The pattern of employment of PhD scientists in Australia has been examined more closely in recent years, particularly in some disciplines. The numbers of graduates are fewer, the PhD is more professionally focused than any first degree and the traditional avenues of employment are fewer. But, just as for the first degree, reliable information is in relatively short supply and no Australia-wide figures giving details of employment are available.

What is clear is that in the early 1970s the production of PhD scientists in most fields exceeds the capacity of the academic labour market to absorb new staff. As the PhD is seen primarily as a professional preparation for those entering university teaching or full-time research work, employment problems inevitably ensue. Stories of 100 or 150 applications for university posts abound and epitomize the situation—a vastly increased number of PhDs chasing a few academic jobs. This situation is familiar in all English-speaking countries.

It must be remembered that the PhD is very recent in Australia and that the number of awards quadrupled in the 1960s. The urgent needs of the universities themselves have been met, as have those of government-supported research (particularly through the Commonwealth Scientific and Industrial Research Organization, CSIRO). A stage of reassessment now seems to have been reached, when it is difficult for some PhD graduates to get jobs and when there is some evidence that students are unwilling to enrol for PhDs as knowledge of these difficulties becomes publicized (sometimes inaccurately). Again, the precise extent of these changes has not been recorded and at the moment it is only an educated guess that the rate of increase in the enrolment for the PhD has fallen.

Some reasons for employment difficulties are clear, such as the flow-on effect of similar difficulties in other countries, and the levelling off of university growth within Australia. There has been a continuing debate about the level of participation by Australian PhD graduates in industrial research and development and in non-scientific employment. What is certain is that Australian industry tends overall to enjoy a fairly low level of investment, of both finance and manpower, in research and it has consequently been difficult for the initial "overflow" of PhD scientists to be absorbed into jobs in industry which they prefer. Added to this is the simple novelty of the idea that there are PhD graduates looking for jobs outside education and research.

From the mid-1960s on, there has been a number of studies of the factors affecting the supply and demand situation for PhDs. Armstrong, Hill and Ross⁴ foresaw the "overflow" problem. Parker *et al.*¹³ and Middleton *et al.*¹⁴ have examined the situation of PhD chemists in detail. West^{15,16} has looked at the general question of the use of scientific manpower, and has linked this to a consideration of a national science policy, an area which is relatively untouched in Australia. The Commonwealth government now has a Department of Science, and scientific manpower questions are part of that department's concern; for the moment, however, the valuable work done by those mentioned and others still adds up to a rather piecemeal picture.

Recently the Standing Committee on Science and Education of the Science and Industry Forum of the Australian Academy of Science has received a report¹⁷ which makes a more comprehensive attempt at understanding attitudes towards PhD training and employment in Australia. The study, as did several earlier ones, focuses on chemists; it also contains a comprehensive review of relevant previous studies and writings in Australia. The importance of chemistry in the PhD picture can be seen from Table 2, which gives a breakdown of PhDs awarded in science in 1972.

The Academy of Science study looks very closely at the nature of PhD training in science in Australia, at the general problems of understanding the market for highly qualified

scientists and at the attitudes which exist amongst students towards the employment market. It is clear that the often-assumed prejudice against employment in industry and in non-tertiary teaching does exist and that many PhD students themselves (more than 40%) are worried about employment prospects. The definition of "professionalism" which students have absorbed throughout their careers virtually requires that

Table 2 Australian PhD Degrees in Science in 1972*

	% of total
Applied science	9
Biological science	16
Chemistry	56
Physics	11
Geology	4
Mathematics	4

* Compiled from material supplied by the Bureau of Census and Statistics.

they work within a university environment for professional fulfilment.

This report, thorough as it is, in a sense simply points the way to a need for an even more searching investigation of the relationships between PhD study and employment. We know there are problems of overflow from the traditional areas of employment and difficulties in gaining acceptance, on both sides, of the employability of PhD graduates in industry. We do not know the precise dimensions of the problem. In 1973, for the first time, the Graduate Careers Council of Australia will attempt to collect information for PhD graduates as it does for first-degree graduates, indicating not only the pattern of their first destination but also the extent of unemployment and underemployment. Just how this information, together with other data, will be used by students, university planners and employers remains to be seen. In the meantime it can simply be said that Australian PhD scientists can face a real employment problem, given the nature of their training and the prevailing attitudes in the job market.

The Future

In Australia, graduates themselves have begun to show a deep interest in their prospects of employment. This interest is more likely to be articulated by smaller well-identified groups such as PhD graduates in chemistry or physics who will see themselves as professional groups. First-degree science graduates are a much more heterogeneous body and will see themselves much less clearly as a professional group or groups.

Consequently few initiatives have been taken by first-degree graduates to analyse and seek an understanding of their employment problems but a number of studies have been undertaken at the initiative of PhD graduates. Most of these studies have been limited to the examination of a particular professional group. Valuable as these limited studies are, it is essential that much more detailed and extensive studies be undertaken of education and employment in Australia for first-degree and higher-degree graduates. The graduates themselves must be encouraged to seek an understanding of their problems but this will not be enough; other bodies must also become increasingly active. Australia-wide studies of education and employment of science graduates at all levels at the depth of the studies prepared for the National Institutes of Health by the National Research Council of the United States¹⁸⁻²⁰ should be undertaken.

In a federation such as Australia, with difficulty in securing nationwide policies on education, we are doubtful about the possibility or the wisdom of the adoption of a central policy to regulate the supply of science graduates to the various outlets. There can, however, be no doubt that the days of

complete *laissez-faire* for universities in the choice of the areas into which they will put their effort to produce graduates (both with first degrees and higher degrees) are over. Increasingly universities will be persuaded and in some circumstances directed by government and government agencies using the power of financial control.

In our opinion, therefore, it will become vital for universities and government to have as much information as possible about the factors involved in the selection by students of university courses, the attitudes of universities in the offering of courses and the attitudes of employers in making use of the graduates from those courses. The more complete and reliable this information is, the less likely it is that there will be conflict between government, the universities and the employers.

There are some very good signs for the future. Government, particularly the Commonwealth government, has already asked the Australian Universities Committee to make a careful study, in collaboration with the departments concerned with labour, science and education, of the factors involved in determining the demand for particular types of graduates. The universities and the Australian Vice-chancellors' Committee, in collaboration with the Australian Academy of Science, have undertaken a deep and careful study of the education and employment of science PhDs and the Graduate Careers Council of Australia following a conference entitled "Graduates for What?", held in Canberra in August 1972, has received Commonwealth government support for a continuing survey of the first destinations of Australian university graduates. The appointments boards of Australian universities continue to use their limited reserves to conduct surveys of supply and demand for graduates.

It must be recognized that worthwhile studies would probe the question of the basic objectives of many university courses and may disturb many universities which are too ready to rely on tradition and hunch in the decisions they make concerning the continuation of their existing courses and the introduction of new ones.

¹ *University Statistics Part I—Students and Degrees Conferred* (Bureau of Census and Statistics, Canberra, published annually).

² *Report of the Committee on Australian Universities* (Murray Report) (Canberra, 1957).

³ Gani, J., *The Condition of Science in Australian Universities* (Pergamon Press, London, 1963).

⁴ Armstrong, P. N. G., Hill, Stephen C., and Ross, I. G., *Australian PhD Graduates in Science and Applied Science*, Proc. RACI (June 1966).

⁵ Armstrong, P. N. G., Hill, Stephen C., and Ross, I. G., *Australian PhDs, Specialization, Supply, Demand*, Proc. RACI (July 1966).

⁶ Gravell, K., and Rawling, S. J., *Destinations of University Graduates* (Melbourne, 1972).

⁷ Argy, F., *Australian Physicist*, 7, 3 (1970).

⁸ Australian Biochemical Society, *Newsletter* (September 1972).

⁹ Department of Primary Industry, *Survey of Trained Manpower for Australian Agriculture* (Canberra, 1971).

¹⁰ Kidd, G. A., *Graduate Careers Directory*, Part II (Melbourne, 1970).

¹¹ Townley, K. A., McLeod, I. R., and Conybeare, C. E. B., *A Survey of Geoscientists in Australia, 1967* (Geological Society of Australia, 1968).

¹² Rawling, S. J., in *Graduates for What? Proceedings* (Canberra, in the press).

¹³ Parker, A. J., et al., *Chemistry PhDs—Supply and Demand* (Report by Career Sub-Committee: Faculty of Research School of Chemistry, Australian National University, Canberra, 1970).

¹⁴ Middleton, B. S., et al., *The Employment of Chemistry PhDs in Australia*, Proc. RACI (1971).

¹⁵ West, C. E., *Search*, 3, 9 (1972).

¹⁶ West, C. E., *Search*, 3, 10 (1972).

¹⁷ Hill, Stephen C., Fensham, Peter J., and Howden, Ian B., *The Education of PhD Scientists in Australia and its Implications for Employment* (Australian Academy of Science, Canberra, in the press).

¹⁸ *Profiles of PhDs in the Sciences* (National Academy of Sciences, Washington DC, 1965).

¹⁹ *Careers of PhDs: Academic vs Non-academic* (National Academy of Sciences, Washington DC, 1968).

²⁰ *Mobility of PhDs Before and After the Doctorate* (National Academy of Sciences, Washington DC, 1971).

Stratigraphical Relationships of the Plio-Pleistocene Deposits, East Rudolf, Kenya

BRUCE E. BOWEN & CARL F. VONDRA

Department of Earth Science, Iowa State University, 53 Science Hall, Ames, Iowa 50010

This report summarizes the stratigraphical relationships of the Plio-Pleistocene sediments at East Rudolf and formalizes the stratigraphical nomenclature.

THE geological investigation of the area east of Lake Rudolf, northern Kenya, was initiated in 1969 by A. K. Behrensmeyer and continued by a team from Iowa State University in 1970, 1971 and 1972 as a part of the National Museums of Kenya expedition. This report summarizes the stratigraphical relationships established during the 1971 and 1972 seasons of the Plio-Pleistocene sediments and formalizes the stratigraphical nomenclature.

In 1968 the National Museums of Kenya, under the direction of R. E. F. Leakey, organized an expedition to the north-eastern shore of Lake Rudolf. The purpose of the expedition was to determine the palaeontological potential of the sediments observed by Mr Leakey in an earlier aerial reconnaissance. Subsequent work has shown that the Plio-Pleistocene sediments did indeed contain fossils and to date more than eighty hominid fossils, a wealth of late Tertiary and Pleistocene mammalian fossils and a series of archaeological sites have been discovered.

The East Rudolf Basin is located at the northern end of the Gregory Rift Valley. The main graben forms the Suguta Valley at the southern end of Lake Rudolf and trends across the lake in a north-northeasterly direction. The northward continuation of the trend is the Kinu Sago fault belt east of Lake Rudolf and the Stephanie graben on the Ethiopian border¹ (see Fig. 1). The area studied lies on the northwestern flanks of this trend.

The Suregei cuesta forms the eastern margin of the East Rudolf Basin (see Fig. 2), and consists of a series of interbedded Miocene basalt flows, associated palaeosols and sediments which were uplifted and tilted westward by early Pliocene faulting. The basin is transected by the northeast-southwest trending Kokoi structure, which consists of Pliocene basalt flows and interbedded lacustrine sediments recently faulted and uplifted by northward trending faults. Faults in the area, although numerous near the lake, are of relatively minor importance; they form a series of small half-grabens and horsts with a general northward trend. Except for reversals caused by faulting, the sediments dip gently toward the lake.

Correlation

The sedimentary exposures in the East Rudolf Basin were originally separated into three areas because of the difficulty of stratigraphical correlation between them³. The exposures at Ileret, the northernmost area, are separated from those along the Koobi Fora ridge, the central area, by the Kokoi structure and a large Holocene alluvial plain complex to the east of the Kokoi. The Holocene floodplain deposits of the Laga Bura Hasuma separate the Kubi Algi area, the southernmost, from the Koobi Fora area (see Fig. 3).

During the 1971 and 1972 field seasons strata were measured and described along the western slope of the Suregei cuesta

at the eastern margin of the Ileret, Koobi Fora and Kubi Algi areas. Marker beds were established and traced around the Kokoi and the head of Laga Bura Hasuma into the previously described exposures at Ileret and Koobi Fora³.

The oldest rocks exposed in the East Rudolf Basin are the basalt flows (11.6 ± 0.5 m.y. BP and 14.1 ± 1.4 m.y. BP)³ of the Suregei cuesta. In the northern and central sequences a basal conglomerate consisting of basalt cobbles and pebbles in a pale yellow-brown claystone matrix non-conformably overlies the basalts. It grades laterally into a very pale orange laminated claystone which contains 5–8 m thick lenses of basalt boulders which range up to 1 m in diameter. A series of grey-orange, fine to coarse grained, crossbedded sandstones interbedded with three prominent grey tuff horizons conformably overlie the conglomerate and claystones. This entire sequence attains a composite thickness of 155–170 m. The lower portion is near-shore to transitional lacustrine grading upward into a dominantly fluvial sequence intercalated with thin transitional lacustrine beds.

The three tuff horizons are the best marker beds available and serve as the basis for correlating the Ileret, Koobi Fora and Kubi Algi sedimentary sequences. The lower, a complex—here designated the Suregei Tuff Complex—can be traced around the Kokoi along the western flanks of the Suregei cuesta. It extends from immediately east of the Ileret area to about 15 km to the south along the Suregei. The Suregei Tuff Complex can be used to correlate the strata measured at the northern end of this traverse with the strata measured at the

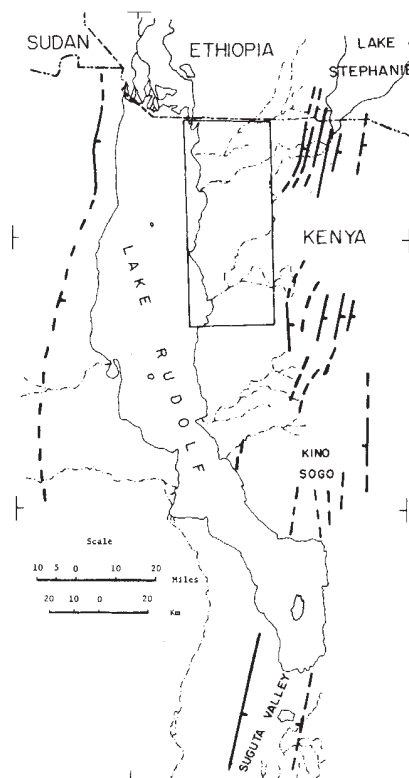


Fig. 1 Index map showing the location of study area and the boundary faults of the Kenya Rift.

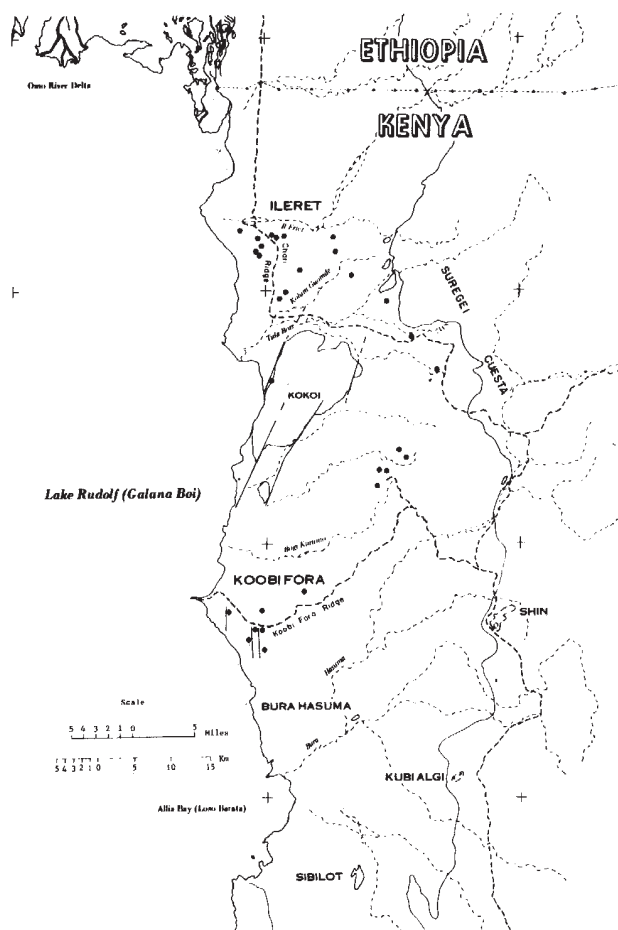


Fig. 2 Map of the study area showing prominent physiographical features and locations of sections.

southern end. The uppermost tuff in the sedimentary sequence at the southern end of this traverse can then be traced along a series of low NE-SW trending escarpments into the sedimentary sequence in the Koobi Fora area. This tuff correlates with the KBS Tuff in the Koobi Fora IIA unit of Behrensmeyer² and Vondra *et al.*³. The sequence of tuffs is essentially the same in the exposures in the Ileret area and those to the south. The intervening strata and the vertical separation and corresponding thicknesses of the tuffs are similar. On this basis the uppermost tuff in the northern sequence has been correlated with the uppermost tuff in the southern sequence and with the KBS Tuff at Koobi Fora with confidence (Fig. 3). The intermediate tuff is here named the Tulu Bor Tuff.

The KBS Tuff and associated strata can be traced southward to the floodplain complex of the Laga Bura Hasuma. The strata below the KBS Tuff are traceable around the Laga Bura Hasuma to a series of ridges immediately south of the floodplain complex of the stream. The exposures previously described by Vondra *et al.*³ are then traceable along a series of terraces into the lower portion of the sedimentary sequence near Kubi Algi. The upper 80 m of these exposures is similar in lithology and number of tuffs to the sedimentary sequences exposed along the Suregei cuesta in the Ileret and Koobi Fora areas. The first tuff below the KBS Tuff can be correlated with the Tulu Bor Tuff and the next lower tuff complex with the Suregei Tuff Complex (Fig. 3).

Nomenclature

With the correlation established it is now possible to formalize the stratigraphical nomenclature. The term Koobi Fora Formation is here proposed to include those sedimentary strata which lie between the basal contact of the Suregei

Tuff Complex and the upper contact of the prominent tuff, exposed along the Ileret ridge, here named the Chari Tuff, or the basal contact of the Holocene grey, tuffaceous, predominantly lacustrine siltstones which represent a late stage rise in the level of Lake Rudolf (Fig. 2). The exposures located near Koobi Fora at 3° 56' N latitude and 36° 15' E longitude (BH6136, East Africa Grid) best illustrate the lithology of the formation and are here designated as the type exposures. This includes the informal Koobi Fora I, II and III units of Behrensmeyer² and Vondra *et al.*³, and the lower unit at Ileret as defined and described by Vondra *et al.*³.

Those beds in the Ileret area formally designated the "lower unit"³ are a lithologically homogeneous sequence of fine grained sandstones and siltstones. They are geographically restricted to the Ileret ridge and as yet cannot be correlated with the sequence at Koobi Fora. Therefore, the term Ileret Member is proposed for the beds which lie between the upper contact of the KBS Tuff in the Ileret area and the top of the Chari Tuff which caps the Koobi Fora Formation. The outcrops located in the Ileret area at 4° 16' N latitude and 36° 15' E longitude (BH6173, East Africa Grid) are designated as the type exposure.

Informal member designation is applied to the Koobi Fora Formation in the Koobi Fora area until the lateral relationships in the upper part of the formation can be definitely established. The lower portion of the formation between the basal contact and the top of the KBS Tuff is designated as the lower member and the strata above the KBS Tuff are referred to as the upper member.

Those strata that lie below the basal contact of the Suregei Tuff Complex are here designated as the Kubi Algi Formation. The outcrops located along a terrace trending toward Kubi Algi at 3° 45' N latitude and 36° 19' E longitude (BH6915, East Africa Grid) are designated as the type exposure.

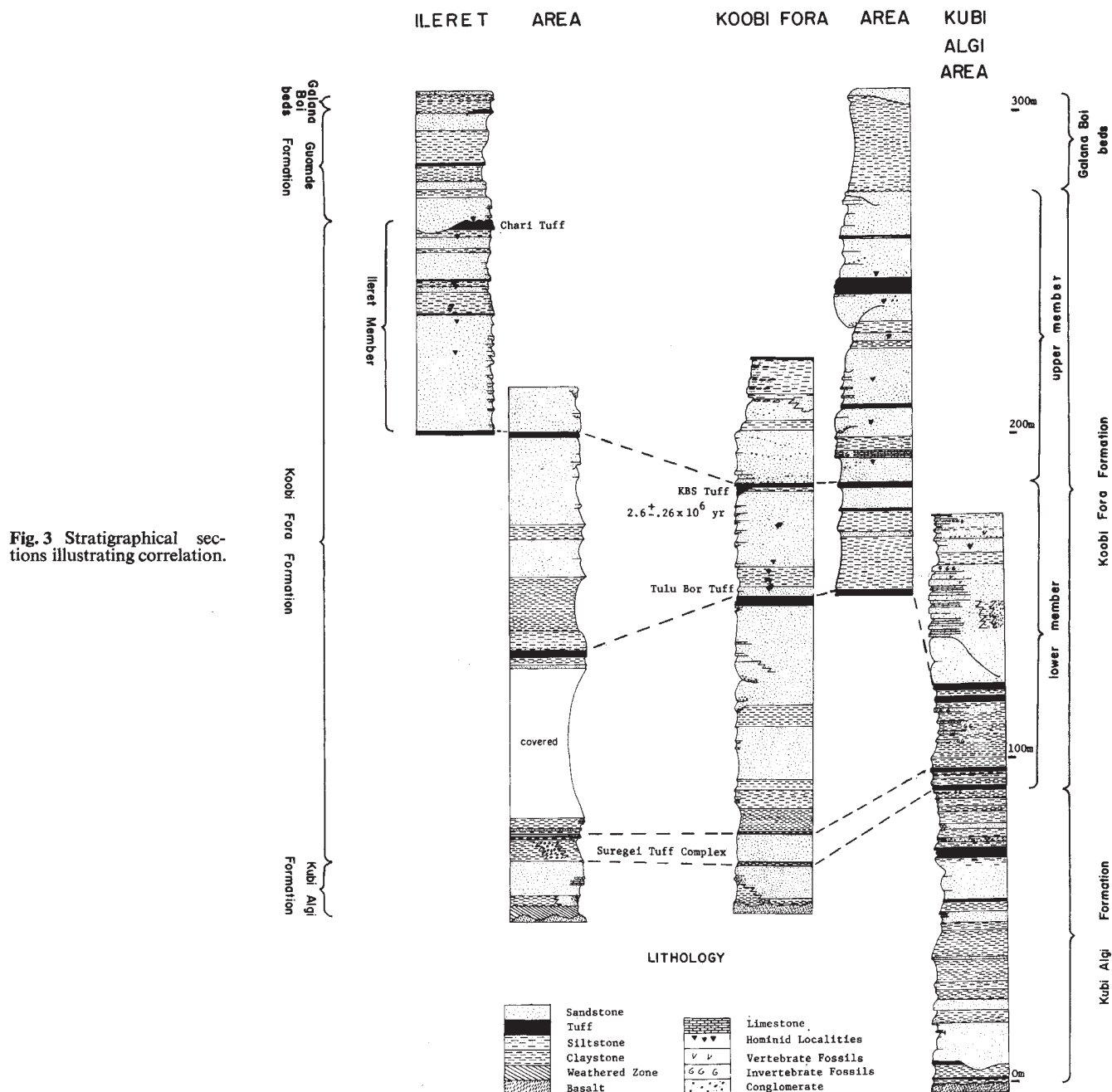
The Guomde Formation is proposed to include those strata which lie between the top of the Chari Tuff on the Ileret ridge and the overlying grey tuffaceous siltstones which represent a late stage rise in the lake level. The upper contact of the Guomde is marked by a 1 m thick tuff. The exposures located on the Ileret ridge at 4° 18' N latitude and 36° 15' E longitude (BH376, East Africa Grid) best illustrate the lithology of the formation and are designated as the type section. The Guomde Formation comprises the middle unit at Ileret as described by Vondra *et al.*³.

The term Galana Boi is retained for the grey, tuffaceous siltstones which cap the Guomde Formation at Ileret and applied to the fourth unit recognized in the Koobi Fora area by Vondra *et al.*³. Although the lithology and stratigraphical position are the same for the late stage lacustrine beds at Ileret and Koobi Fora their exact lateral relationships are not yet fully known.

The environments of deposition represented by the Kubi Algi, Koobi Fora and Guomde Formation and the Galana Boi beds are currently being worked out so that only a preliminary summary of the palaeogeography can be presented. Detailed lithological description and depositional history of the units proposed along with aerial geological maps showing their distribution will be published in the near future.

The sediments of the Kubi Algi and Koobi Fora formations are interpreted to represent a prograding deltaic complex. At the margins of the basin, thin lenses of near shore lacustrine sediments are intercalated with the predominantly deltaic plain and fluvial deposits documenting periodic lacustrine transgressions. The sediments tend to coarsen upward in the centre of the basin indicating delta growth and regression through time. Periodic coarsening, large-scale cut and fill structures and angular relationships with underlying sediments are characteristic of the basin margin fluvial deposits. These features are attributed to the elevation of the source area and downwarping of the lake basin as a result of periodic tectonic activity along the rift system.

Mammalian fossils occur throughout the Kubi Algi and



Koobi Fora formations, but an abundant representative sample is preserved only in the upper portion of the lower member and in the Ileret Member of the Koobi Fora Formation⁶. This sample is the basis for the faunal zonation of the Koobi Fora Formation⁶. Hominid fossils usually occur in the upper portion of the Koobi Fora Formation. The localities are indicated in Fig. 3. Hominid KNM-ER 1470, recently described by R. E. F. Leakey^{7,8}, was probably eroded from a sandstone of deltaic plain origin in the upper portion of the lower member of the Koobi Fora Formation, 35.5 m below the KBS Tuff.

The Guomde Formation, which overlies the Koobi Fora Formation with angular unconformity, represents a limited transgression of the lake in the Ileret area. The sediments were deposited predominantly in nearshore lacustrine and deltaic plain environments.

The Galana Boi beds, which unconformably overlie the Plio-Pleistocene sediments, range from coarse-grained fluvial deposits to fine-grained nearshore lacustrine silts. They occur up to 120 m above the present lake level. The sediments document both a major lacustrine transgression around 9,200

years BP and significant BP tectonic activity post-9,200 years.

This work was supported by a grant from the National Science Foundation to C. F. V. and in part by grants from the National Geographic Society and the L. S. B. Leakey Foundation to R. E. F. Leakey. The work was greatly aided by the cooperation and assistance of the National Museums of Kenya and the Kenyan Government. We especially thank R. E. F. Leakey for his valuable assistance and hospitality and members of the East Rudolf Research Group, particularly G. L. Isaac, A. K. Behrensmeyer, G. D. Johnson and I. C. Findlater for their assistance and many useful suggestions.

Received May 22, 1972; revised January 15, 1973.

¹ Baker, B. H., *Spec. Pap. Geol. Soc. Amer.*, 136 (1972).

² Behrensmeyer, A. K., *Nature*, 226, 225 (1970).

³ Vondra, C. F., Johnson, G. D., Bowen, B. E., and Behrensmeyer, A. K., *Nature*, 231, 245 (1971).

⁴ Leakey, R. E. F., *Nature*, 226, 223 (1970).

⁵ Leakey, R. E. F., *Nature*, 231, 241 (1971).

⁶ Maglio, V. E., *Nature*, 239, 379 (1972).

⁷ Leakey, R. E. F., *Nature*, 242, 170 (1973).

⁸ Leakey, R. E. F., *Nature* (in the press).

LETTERS TO NATURE

PHYSICAL SCIENCES

Redshift of OH471

THE purpose of this letter is to draw attention to the very high redshift, $z=3.40$, of the QSO OH471.

The position and identification of the radio source OH471 were communicated to us by C. Hazard and are based on the accurate positional work of the group at the Royal Radar Establishment. The source had previously been identified by Gearhart, Lund, Frantz and Kraus¹ who also publish a finding chart. The position coordinates are

$$\alpha_{1950} = 06 \text{ h } 42 \text{ min } 52 \text{ s}$$

$$\delta_{1950} = +44^\circ 54' 33''$$

On the Palomar Sky Survey the identification is a neutral, stellar object of estimated magnitude $m_B \sim 18\text{--}18.5$ mag.

The object has been studied spectroscopically with the Cassegrain image tube spectrograph at the Steward Observatory 90 inch telescope. Two spectrograms (SI 830a, 835a) have been obtained covering the range 3200–5800 Å at a dispersion of ~ 240 Å/mm. They both clearly show strong emission features at 4548 Å and 5351 Å, the former being somewhat weaker but broader than the latter. The ratio of measured wavelengths (1 : 1.177) agrees well with the rest wavelength ratio (1 : 1.176) of OVI $\lambda 1033.8$ and L α . Among strong emission lines normally observed in QSOs, no other ratio of rest wavelengths falls close to this value. We therefore tentatively identify the feature at 5351 Å with L α and that at 4548 Å with OIV (1031.9, 1037.6) possibly blended with a contribution from L β . The redshift of OH471 would then be $z=3.40$, substantially greater than the previous highest redshift $z=2.88$ for 4C5.34 (ref. 2).

Although our redshift determination rests primarily on these two emission lines, the derived value of z is supported by the following additional evidence:

(i) According to Lynds³ OVI is the dominant emission feature shortward of L α in the spectrum of 4C5.34.

(ii) An emission feature at ~ 5460 Å seems both too strong and too wide to be attributed solely to the night sky Hg $\lambda 5461$ line. It would, however, correspond to NV $\lambda 1240$ at the derived redshift. A third spectrogram SI 840 covering the range $\lambda 5000\text{--}7200$ Å at a dispersion of 180 Å/mm, although severely underexposed because of intermittent cloud, showed evidence of a weak emission feature near 6820 Å which would correspond to CIV $\lambda 1550$ at the suggested redshift. Both of these lines, however, require confirmation.

(iii) The spectrum of OH471 is rich in absorption lines suggesting a high redshift, $z > 1.7$, say. If the feature at 5351 Å were identified with another, normally weak, line instead of with L α , it would follow that emission L α must be absent altogether from the spectrum. For example, SiIV $\lambda 1397$ and HeII $\lambda 1640$ have a wavelength ratio 1 : 1.174 and could therefore be identified with the observed emission features; the corresponding redshift would then be $z=2.26$. The absence of strong features at 3966 Å and 5054 Å corresponding to L α and CIV $\lambda 1550$ respectively would then require explanation.

(iv) Although higher resolution spectrograms are required for a detailed analysis of the absorption line spectrum (an observational programme to this end is under way at Steward Observatory), one absorption system can probably be discerned on the well-widened spectrogram SI 835a. Strong, fairly broad, absorption features were found at 5281, 4225, 4126 Å and may be identified with L α , L γ and L δ at a redshift z_A of 3.3440, 3.3444 and 3.3445 respectively. There is a broader, strong feature centred at 4462 Å which may be due to a blend of L β and a line (or lines) in another absorption system. Finally, there is a rather weaker blend centred at 4080 Å which may contain a contribution from L ϵ in the same system. Confirmation of this system must, however, await further spectroscopic evidence.

(v) The continuum of OH471 seems to decrease sharply shortward of ~ 4000 Å corresponding approximately to the redshifted wavelength of the Lyman limit (912 Å) in the emission system. Indeed both blue region spectrograms seem to be dominated by the night sky emission shortward of ~ 3900 Å, by contrast with spectra of other objects taken with the same system; this would be consistent with strong absorption in the Lyman continuum. Continuum distributions are, however, notoriously difficult to assess from photographic material and this point also requires confirmation using some form of linear detection system and proper sky subtraction.

On the basis of this additional evidence we feel that the line identifications and the consequent high redshift $z=3.40$ are probably correct.

If QSO redshifts are due mainly to Hubble expansion OH471 is the most distant object so far known as well as being among the most intrinsically luminous. Its presence at $z=3.40$ and $m_B \sim 18\text{--}18.5$ argues against explanations of the apparent paucity of QSOs with redshift exceeding 2.2 in terms of intergalactic obscuration at earlier epochs. The neutral colour does, however, raise the question of whether the redshift dependence of observed colour for a fixed intrinsic spectrum^{4–6} introduces selection effects that work against discovery of high redshift objects⁷. With the extremely accurate radio positions now available (to which the identification of OH471 in a crowded star field bears ample testimony), this question can be settled observationally. There is, however, little evidence at present against the view expressed by Lynds and Wills² that the apparent dearth of high redshift QSOs is due to their intrinsic rarity.

We thank Dr C. Hazard and members of the group at RRE, Malvern, for communicating their accurate radio positions and identifications. We also thank Drs C. R. Lynds, R. J. Weymann and N. J. Woolf for helpful discussions. Research at Steward Observatory has been supported by the NSF.

R. F. CARSWELL
P. A. STRITTMATTER

Steward Observatory,
University of Arizona,
Tucson, Arizona 85721

Received March 5, 1973.

¹ Gearhart, M. J., Lund, J. M., Frantz, D. J., and Kraus, J. P., *Astrophys. J.*, **77**, 557 (1972).

² Lynds, R., and Wills, D., *Nature*, **226**, 532 (1970).

- ³ Lynds, R., *Astrophys. J. Lett.*, **164**, L73 (1971).
⁴ McCrea, W. H., *Publ. Astron. Soc. Pacific*, **78**, 49 (1966).
⁵ Strittmatter, P. A., and Burbidge, G. R., *Astrophys. J.*, **147**, 13 (1967).
⁶ Bahcall, J. N., and Sargent, W. L. W., *Astrophys. J. Lett.*, **148**, L65 (1967).
⁷ Browne, I. W. A., and McEwan, N. J., *Nature Physical Science*, **239**, 101 (1972).

Hydrogen Flash in Stars

DURING the past few years there has been some progress in understanding the way in which a star approaches the main sequence. In their studies of the pre-main sequence evolution of stars, Ezer and Cameron¹ assumed that the stellar material started with the highest possible adiabat consistent with the virial theorem (for a given temperature a high adiabat has a low density and a low adiabat has a high density). In later hydrodynamic collapse studies of the formation of a star with zero angular momentum, Hayashi² and Larson³ showed that much of the internal energy of the collapsing gas is radiated away, so that the stellar material must start on a much lower adiabat. The gas in the centre of such an object can only be heated by compression, so that it remains on a low adiabat. As the outer layers of the star fall onto this core, they undergo shock heating, due to gravitational potential energy release, which raises their adiabat, and allows the star to form a stable body in hydrostatic equilibrium prior to reaching the main sequence.

It is unrealistic to expect a star to form with zero angular momentum. Star formation studies⁴ have shown that collapsing interstellar gas should form a flat disk, from which the star should form by gaseous dissipation processes⁵. The gas density near the disk centre is comparable to that in the collapsing core of Larson's models at which the first halt and mild shock heating occurs³. Hence shock heating effects should be relatively unimportant when a flat rotating disk (primitive solar nebula) is formed. The initial low adiabat of the gas in the disk will not be raised during the dissipation, but is likely to be substantially lowered as a result of radiation into space from the photosphere⁵.

We have estimated the initial adiabat of the gas in the disk in several ways. Collapsing interstellar gas undergoes isothermal compression with a temperature of about 10 K as long as the emitted radiation is free to escape from the system^{2,3}. When the density becomes high enough, the subsequent compression is adiabatic, but there is an ambiguity concerning the adiabat, because we do not know the distribution of the hydrogen molecules between the para and ortho forms⁶. We have used Larson's estimate of the temperature and density at which the switch from isothermal to adiabatic compression begins, and have computed the adiabat based on two possible limits in the relative number of parahydrogen and ortho-hydrogen molecules.

A third estimate is based on the pressure and temperature conditions deduced for the primitive solar nebula at the time of meteoritic accumulation. Accumulation processes should take place rapidly in the primitive solar nebula⁴, so that the thermodynamic conditions associated with accumulation probably differ rather little from those in the initial nebula. Although there is still considerable uncertainty associated with the meteoritic cosmo-thermometers and cosmobarometers⁷⁻¹⁰, we can take approximately a temperature of 450 K and a pressure of 5×10^{-6} atmos as the conditions defining the adiabat.

We extended these adiabats toward higher temperatures and densities, taking into account the internal energy of excitation of hydrogen molecules, and the dissociation and ionization of hydrogen and helium. Because the adiabats are relatively low, thermal dissociation of hydrogen molecules is not complete before effects of pressure dissociation become important; we

have estimated the lowering of the dissociation energy at higher pressures following Vardya¹¹. The subsequent thermal ionization of the hydrogen atoms is very inefficient; more than half of the hydrogen atoms remain neutral until pressure ionization takes place. At higher densities the ionization energy of hydrogen is lowered through coulomb binding of electrons to the plasma¹², but pressure ionization results chiefly from the partial shielding of the K-shell electrons in the neutral atoms by free electrons inside the classical Bohr radius¹³. The final adiabat is relatively insensitive to crude approximations in this treatment.

After ionization is complete, the adiabat can be written in the conventional form $P = K\rho^{5/3}$. With the pressure and density expressed in c.g.s. units, we found for our three estimates of the initial adiabat, $K = 1.08 \times 10^{13}$, 1.67×10^{13} and 2.61×10^{13} . The first two of these values correspond to the Larson estimate for the onset of adiabatic compression, the first when hydrogen molecules are all in the parahydrogen form, and the second when the para/ortho ratio is 1/3. The third value corresponds to meteoritic accumulation conditions. Because it represents the highest adiabat, our subsequent discussion is based on this third value.

We consider a star which has this initial adiabat everywhere in its interior structure. Such a star can be represented¹⁴ by a polytrope of index 1.5. The central conditions are determined through specification of the total mass M and the adiabatic constant K . For the centre of such a polytrope we find a temperature of 7.0×10^7 K and a density of $7,200 \text{ g cm}^{-3}$. These values are much higher than now exist at the centre of the Sun. For the present central solar temperature of 1.5×10^7 K, the density on the adiabat is 700 g cm^{-3} , also much higher than the present central density of the Sun. Further lowering of the adiabat by radiative cooling would increase these discrepancies.

So the Sun is unlikely to have formed as a spherically-symmetric body in hydrostatic equilibrium before the onset of hydrogen-burning at the centre. The ignition of hydrogen thermonuclear reactions evidently took place while the central portion of the solar nebula still formed a flattened rotating disk. The nuclear energy released during a possible preceding phase of deuterium burning is too small to modify this conclusion. If we take the D/H ratio in the solar nebula to be 10% of that in the oceans, the density at a given temperature would only be decreased by about 30% during the deuterium-burning process. The dissipation processes leading to the hydrogen ignition should take only a few thousand years⁵.

It is a property of a self-gravitating infinite plane distribution of matter that the pressure at the central plane is independent of the temperature⁵. This resembles the situation in an electron-degenerate stellar core. Thus it is possible that the ignition of hydrogen-burning reactions at the centre of the disk can be accompanied by a thermal runaway. This is likely to raise the adiabat of the gas to a higher value than it would attain on the main sequence, expanding the star and forming a body in hydrostatic equilibrium somewhat on the low temperature side of the main sequence in the Hertzsprung-Russell diagram. There would be a rapid rise in the luminosity at this time. This process may explain the sudden flare-up of the star FU Orionis¹⁵.

The process may also account for hitherto puzzling features of the Hertzsprung-Russell diagrams of young clusters. In such clusters the lower luminosity stars are displaced somewhat to the low temperature side of the main sequence. Even the very low luminosity stars are remarkably close to the main sequence; this has seemed to require that they formed many millions of years prior to the higher mass stars^{16,17}. But the dissipation times in flat rotating disks having a wide range of masses are probably all comparable to a few thousand years, less than the dispersion in the interstellar collapse times, and the hydrogen flash process then places the resulting stars all quite close to the main sequence.

This research has been supported in part by grants from the

National Science Foundation and the National Aeronautics and Space Administration.

FAUSTO PERRI*
A. G. W. CAMERON

Belfer Graduate School of Science,
Yeshiva University,
New York, New York
and
NASA Goddard Institute for Space Studies,
New York, New York

Received November 1, 1972.

* On leave of absence from Department of Mathematics, University of Rome, Rome, Italy.

- ¹ Ezer, D., and Cameron, A. G. W., *Can. J. Phys.*, **43**, 1497 (1965).
- ² Hayashi, C., *Ann. Rev. Astron. Astrophys.*, **4**, 171 (1966).
- ³ Larson, R. B., *Mon. Not. Roy. Astron. Soc.*, **145**, 271 (1969).
- ⁴ Cameron, A. G. W., *Icarus* (in the press).
- ⁵ Cameron, A. G. W., and Pine, M. R., *Icarus* (in the press).
- ⁶ Field, G. B., in Field, G. B., Somerville, W. B., and Dressler, K., *Ann. Rev. Astron. Astrophys.*, **4**, 207 (1966).
- ⁷ Keays, R. R., Ganapathy, R., and Anders, E., *Geochim. Cosmochim. Acta*, **35**, 337 (1971).
- ⁸ Laul, J. C., Keays, R. R., Ganapathy, R., Anders, E., and Morgan, J. W., *Geochim. Cosmochim. Acta*, **36**, 329 (1972).
- ⁹ Laul, J. C., Ganapathy, R., Anders, E., and Morgan, J. W., *Geochim. Cosmochim. Acta* (in the press).
- ¹⁰ Jeffery, P. M., and Anders, E., *Geochim. Cosmochim. Acta*, **34**, 1175 (1970).
- ¹¹ Vardya, M. S., *Mon. Not. Roy. Astron. Soc.*, **129**, 345 (1965).
- ¹² Stewart, J. C., and Pyatt, K. D., *Astrophys. J.*, **144**, 1203 (1966).
- ¹³ Rouse, C. A., *Phys. Rev.*, **159**, 41 (1967).
- ¹⁴ Chandrasekhar, S., *Introduction to the Study of Stellar Structure* (Univ. Chicago Press, Chicago, 1939).
- ¹⁵ Herbig, G. H., *Vistas in Astron.*, **8**, 109 (1966).
- ¹⁶ Iben, jun., I., and Talbot, R. J., *Astrophys. J.*, **144**, 968 (1966).
- ¹⁷ Ezer, D., and Cameron, A. G. W., *Astrophys. Space Sci.*, **10**, 52 (1971).

Depositional Histories of Sand Grains from Surface Textures

THE use of the electron microscope to determine the depositional histories of sand grains is a relatively new but well acclaimed technique¹⁻⁶. The underlying principle is that a detrital quartz grain will bear surface textures that may be related to processes that have operated on it during transportation and deposition.

In 1968 Krinsley and Donahue⁴ published micrographs with very full descriptions intended as a glossary of submicrographic sand surface textures. Littoral (beach), aeolian (dune), glacial and diagenetic textures were outlined based on the work of 8 years involving more than 400 sand samples and 4,000 grains. Where possible laboratory duplication of textures was carried out. Krinsley and Donahue stressed the importance that an assemblage of textures should characterize any one abrasive process—a single texture cannot be considered completely diagnostic. An attempt was also made to relate the textures observed to the mechanical or chemical process involved.

I do not intend to undermine the valuable work of Krinsley and Donahue⁴, but suggest here some possible reservations in the interpretation of sand grain surface textures.

During the course of an investigation by scanning electron microscope of some Pleistocene sands of controversial origin from NE Cheshire, sand samples were also selected from known environments to act as standards for comparison with the Pleistocene sands. Among these were quartz grains of primary origin from freshly weathered granite in Northumberland and collected from a stream which drained only the

granite; beach sands from Studland Heath in Dorset, eroded from Cretaceous Lower Greensand; and freshly weathered Carboniferous Millstone Grit grains from the Goyt River valley, Derbyshire. Carboniferous sandstones are believed to be the principal source rocks of the Pleistocene sands⁷. Because all the standard samples, except the beach sands, were freshly weathered, and the beach sands were collected from an area devoid of glacial deposits, there should have been no indication of glacial abrasion. But the surfaces of very many of these grains frequently show textures closely resembling those described as glacial⁴. Fig. 1 is a micrograph of a primary quartz grain from the Northumberland granite, showing high angularity, high relief and arcuate, semi-parallel fractures. Fig. 2 shows a freshly weathered Millstone Grit grain; the same features are clearly visible. Fig. 3, a micrograph of part of the surface of a quartz grain collected from the beach at Studland Heath, Dorset, shows an arcuate, parallel fracture pattern, with rounding of the fracture probably the result of marine abrasion. A similar feature on a quartz grain from a subantarctic deep sea core has been described by Margolis and Kennett⁸ as being produced in a glacial environment and suffering subsequent rounding in an aqueous environment.

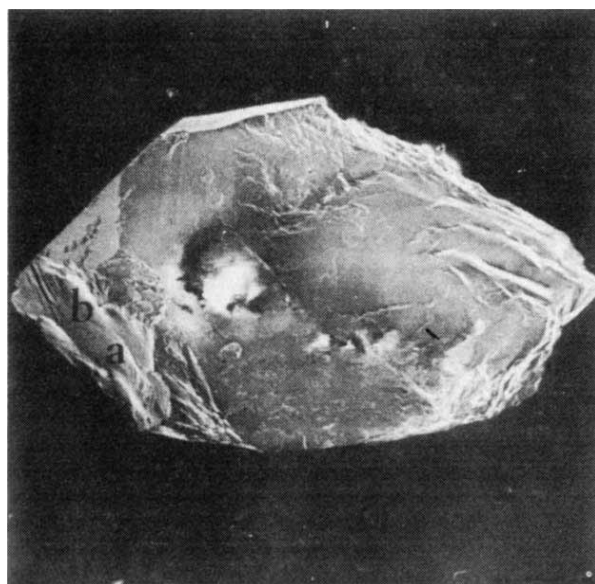


Fig. 1 Micrograph of a quartz grain freshly weathered from Cheviot granite, Northumberland. The grain shows high angularity, fairly high surface relief and both arcuate (a) and parallel (b) fracture patterns. ($\times 56$.)

It thus seems possible that features described as glacial by the earlier criteria⁴ may be found on grains from a variety of environments and which have suffered no glaciation.

Krinsley and Margolis⁵ stated (with reference to aeolian surface textures): "The meandering pattern results from the intersection of slightly curved conchoidal breakage patterns; the conchoidal pattern is probably caused by grain to grain collision in an aeolian medium. These breakage patterns differ from their glacial counterparts in that they are smaller than the largest glacial breakage pattern and have greater uniformity; they differ from those produced by beach action in that the area enclosed is somewhat rounded and more uniform."

There is no real indication here how to distinguish between small glacial fractures and aeolian fractures. Similarly it is probable that a fracture of any origin will tend to become rounded and the area enclosed uniform if subjected to a period of aqueous abrasion.

Krinsley and Margolis⁵ have related the characteristic glacial features of angularity, high relief and large variation in the size of arcuate breakage patterns to the equally large

variety of particle sizes in the glacial environment and the high energy of the transporting medium. It should therefore follow that such features may be produced in any environment where there is a wide variety of particle sizes and high energy conditions. These may include:

Scree slopes where large blocks fall and shatter, and slopes shift periodically to more stable positions. High energy is intermittent and a wide range of particle sizes is produced.

The upper stretches of rivers and streams where there is turbulent flow after heavy rain and a wide particle size range.

The surf zone of a pebble beach where breaking waves pound sand grains onto larger fragments.

In the first two environments the depth of water is shallow and turbulence is sufficient to exceed the protective cushioning effect.

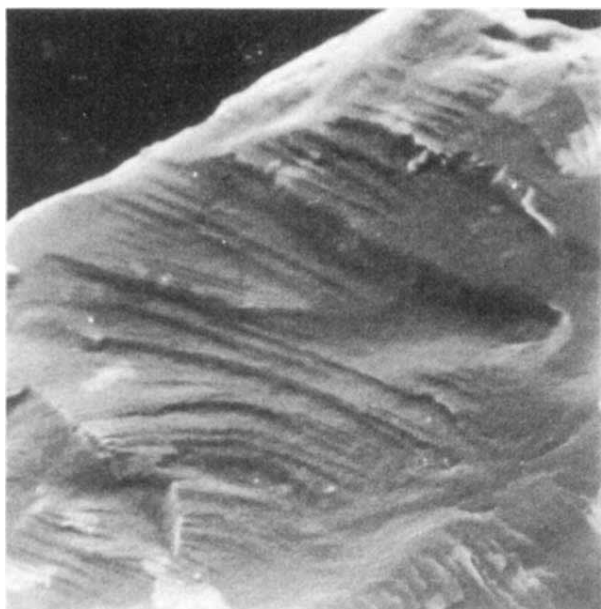


Fig. 2 Micrograph of part of the surface of a quartz grain from freshly weathered Millstone Grit (Goyt Valley, Derbyshire). The micrograph shows several parallel and arcuate fracture patterns. (The apparent rounding of the features is due to incorrect focusing.) ($\times 440$.)

The value of any new sedimentological technique lies in its application to the interpretation of ancient deposits where there is no *a priori* knowledge of transporting medium and depositional environment. The origins of the Pleistocene Middle Sands of NE Cheshire have been the subject of some discussion^{7,9-12}. Possible processes involved during the transportation and deposition of the sands include fluvial, aeolian, glacial, lacustrine (littoral and deeper water) and solifluction. By use of the Krinsley and Donahue⁴ criteria evidence of aqueous, aeolian and glacial abrasion can be distinguished. Fig. 4 is a micrograph of a grain showing fairly typical "glacial" features: high angularity and surface relief, arcuate fractures and surface striations. But this grain was collected from a locality close to the source area of the sands and in a region where glacial drift is evident. Because the transporting medium is believed to have been fluvial^{7,12} the textures observed might have resulted from the initial mechanical weathering of the source rocks; transportation in the upper reaches of a periglacial river turbulent with melt-waters; or abrasion during glacial transportation. From this I conclude that if grains are found bearing apparent "glacial" textures, it is possible to say only that these grains have been subjected to a high degree of mechanical abrasion in an environment of high energy where there is a wide range of particle size present.

Although previous work has shown that there is great value

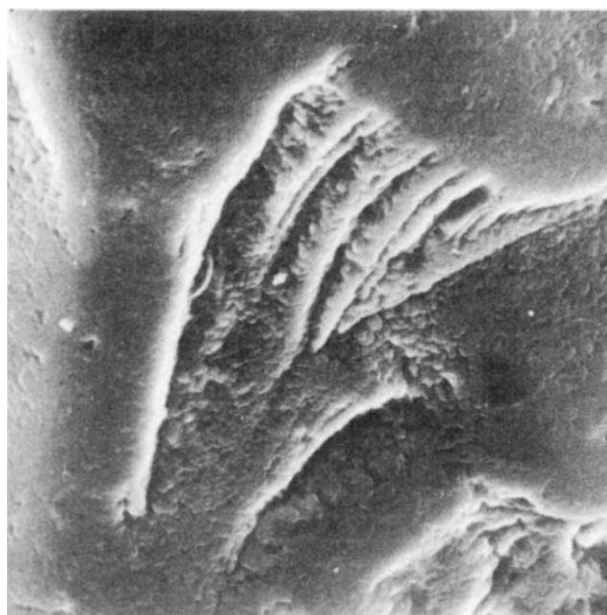


Fig. 3 Micrograph of part of the surface of a quartz grain collected from the beach at Studland Heath, Dorset, and weathered from Cretaceous Lower Greensand. The feature is an arcuate, parallel fracture and rounding is probably the result of marine abrasion. The initial fracture cannot have been produced in a glacial environment. ($\times 920$.)

in the study of quartz grain surface textures as an expression of transportational and depositional processes, perhaps more attention should be paid to the dynamics of such processes to determine what combination of physical parameters is required to produce a particular texture and whether such parameters are replicated in different environments. In the case of simple arcuate and parallel fracture patterns and high relief, the physical requirements may be replicated in many environments. Therefore in the study of a known natural, or simulated, environment the conclusion that the assemblage of textures produced in that environment can be used to isolate the environment is false. It is possible to conclude only that these textures may represent that environment but the pos-

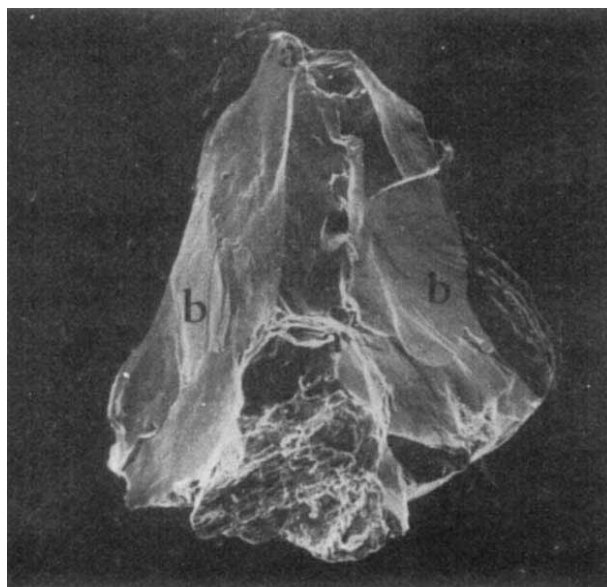


Fig. 4 Micrograph of a quartz grain from Pleistocene Middle Sands of NE Cheshire. This grain is fairly angular, has very high relief and displays many arcuate (a) and parallel (b) fracture patterns. ($\times 88$.)

sibility of other environments with similar dynamic properties cannot be eliminated.

I thank British Industrial Sand Ltd for their permission to collect sand samples from NE Cheshire quarries.

JOAN E. BROWN*

Department of Geology,
University of Manchester,
Manchester M13 9PL

Received January 1; revised January 25, 1973.

* Present address: Ardvreck, 12 Whitefield, Rush Green Road, Lymm, Cheshire.

- ¹ Krinsley, D. H., and Takahashi, T., *Geol. Soc. Amer. Special Paper*, **73**, 175 (1962).
- ² Krinsley, D. H., and Takahashi, T., *Science*, **138**, 162 (1962).
- ³ Krinsley, D. H., and Funnell, B. M., *Quart. J. Geol. Soc. Lond.*, **121**, 435 (1965).
- ⁴ Krinsley, D. H., and Donahue, J., *Geol. Soc. Amer. Bull.*, **79**, 743 (1968).
- ⁵ Krinsley, D. H., and Margolis, S., *Trans. New York Acad. Sci., Ser. II*, **31**, 457 (1969).
- ⁶ Hey, R. W., Krinsley, D. H., and Hyde, P. J. W., *Geol. Mag.*, **108**, 377 (1971).
- ⁷ Brown, J. E., thesis, Manchester University (1972).
- ⁸ Margolis, S., and Kennett, J. P., *Amer. J. Sci.*, **271**, 1 (1971).
- ⁹ Evans, W. B., Wilson, A. A., Taylor, B. J., and Price, D., *Geology of the Country Around Macclesfield, Congleton, Crewe and Middlewich* (Geol. Survey, Memoir, 1968).
- ¹⁰ Simpson, I. M., *Quart. J. Geol. Soc. Lond.*, **115**, 107 (1959).
- ¹¹ Poole, E. G., and Whiteman, A. J., *Quart. J. Geol. Soc. Lond.*, **117**, 91 (1961).
- ¹² Worsley, P., thesis, Manchester University (1967).

Bubble Models in Two and Three Dimensions

THE bubble raft model described by Simpson and Hodgkinson¹ illustrates many of the features of an amorphous alloy. Because the geometrical properties of two and three dimensional space are very different, however, quantitative conclusions drawn from the model must be treated with reserve.

In "crystalline" bubble rafts we see only sections of the one and two dimensional defects (dislocations and grain boundaries respectively), but knowing the lattice repeat in the third dimension we can draw conclusions about the equivalent real crystals. But many properties of the two dimensional model, such as the pair distribution function (PDF), are very different from the three dimensional equivalents, even after maximum correction for obvious geometrical differences.

In a non-crystalline amorphous bubble raft, we know nothing of the molecular arrangements above and below the plane of the model. Moreover, the raft effectively constrains the bubbles to lie in a single plane; this the molecules would not do in reality, but an arbitrary section through the aggregate would intersect most of the molecules within their volumes. Apart from ignoring the third dimension (the characteristics

of which are not implied by the two dimensional case) the constraints on the system result in more symmetry in the raft model than in the real aggregate. Thus, conclusions about three dimensional aggregates must be made reservedly unless the particular property is independent of the dimensionality of the aggregate. Dimensionality dependence of properties is implied, for example, by the Ising model (exactly soluble in two dimensions but not in three²), and the completely different connectivities of planar square and cubic nets, which give very different random walk characteristics³ (of relevance to electrical, thermal and mechanical properties).

Structural differences between aggregates can be discussed in terms of a subdivision of the plane (volume) occupied into polygonal (polyhedral) cells by using the Voronoi construction^{4,5}. The two dimensional raft (or three dimensional packing) is replaced by r space filling polygons each with N_i edges (r polyhedra with N_i faces). Now in two dimensions

$\bar{N} = \frac{1}{r} \sum N_i = 6$ always, independent of the arrangements of spheres⁶; in the ordered "crystalline" raft, all polygons are hexagons, while in the disordered "liquid" case, N_i will vary, but $\bar{N}$ will still equal six. The identity of $N_{2D \text{ crystal}}$ and $N_{2D \text{ solid}}$ is demonstrated by the easy "crystallization" of a two dimensional liquid; the liquid phase is continuous with the solid, there is a "liquid"-"crystalline" critical point, no discontinuity at the melting point, and no supercooling. Thus the two dimensional raft contradicts several important liquid properties. In three dimensions, however, $\bar{N}$ changes discontinuously when the crystal melts, reflecting the existence of supercooling, a discontinuous liquid-solid transition, and the lack of a critical point.

With the above in mind, we can now explain the main features of the PDF's in ref. 1, and show how they relate to the real three dimensional case.

The gross features of any PDF at small distances (say < 3 sphere diameters) reflect those local arrangements of spheres which are possible under the external constraints. Thus we can explain the peak positions of the PDF of a hexagonal close-packed (h.c.p.) raft in terms of the equilateral triangles formed in the plane by the component spheres, the peak heights reflecting the frequency of the triangles and other geometrical figures built up from them.

Fig. 1 shows the intermolecular distances found in the PDF up to 4 diameters. Similarly, a three dimensional h.c.p. PDF can be interpreted in terms of tetrahedral and octahedral sphere arrangements, with peaks at intermolecular distances characteristic of those geometrical figures and their multiples.

The polycrystalline PDF (line 2, Table 1) is in part a superposition of the two PDFs of the hexagonal crystalline arrangements of the two components. Although there will be slight shifting in peak positions and smearing of peak profiles (caused by "grain boundaries" and plotting against the mean diameter of the two components), we can interpret the main features of the PDF in terms of the ordered single component hexagonal raft. Thus we have peaks at distances which correspond closely with the common geometrical distances found in the perfect raft.

Table 1 Comparison of Pair Distribution Peak Positions

Peak number	Dimensionality											
	—	1	2	3	4	5	6	7	8	9	10	11
(1) Hexagonal crystal raft	2	1.00	1.73	2.0	2.65	3.00		3.46		3.61	4.00	4.36
(2) Hexagonal polycrystal raft	2	1.00	1.77	2.01	2.65	3.00		3.49		3.61	3.97	4.41
(3) Amorphous raft	2	1.02	1.88	2.04		2.80		3.50		3.73	3.88	4.48
(4) Random close packing	3	1.00	1.73	1.99			2.65		3.47			4.25
(5) Corresponding vector in Fig. 1	—	1	2	3	4	5	Complex (see text)	6	Complex	7	8	Complex 9

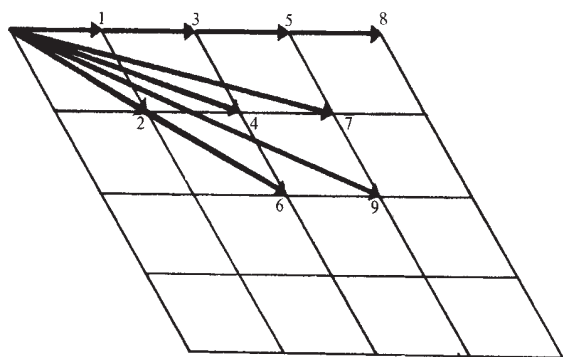


Fig. 1 Vectors of length less than 4.5 sphere diameters in a planar hexagonal lattice. The numbers correspond to those in Table 1 of ref. 1, and also to those in the bottom line of my Table 1.

Partly because the dimensions of the basic geometrical units will vary more (and small proportions of, for example, squares will arise) we expect more PDF blurring for the amorphous bubble raft (line 3, Table 1); we can, however, still interpret the low order peaks in terms of the perfect hexagonal raft (again reflecting the continuity of the two dimensional crystal and amorphous arrays). The peak positions are displaced somewhat from the "crystal" positions, and the peak at 2.80 is the result of a "smearing" together of the "crystal" 2.65 and 3.00 peaks, but in all three cases above, these low order peaks are produced by the same local geometrical arrangements of spheres: equilateral (crystal) and distorted (amorphous) triangles.

Now consider the PDF of the three dimensional random packing (RCP) of equal spheres. The peak positions are plotted on line 4, Table 1, so that only peaks arising from the same geometrical arrangement of neighbours are placed in the same column. Peaks 1, 2 and 3 correspond to peaks in the previous three PDFs because they arise from the same two dimensional arrangements of spheres which of necessity must be present even in an RCP (again, see Fig. 1). Peak 6, however, does not arise from the same source as peak 4, but comes from averaging over several distances between 2.45 and 2.90 in non-crystalline aggregates of tetrahedra (Bernal's pseudonuclei⁷); hence its structural significance is totally different from that of peak 4. From peak 6 outwards the RCP function bears little resemblance to the two dimensional PDFs: peaks 5, 9 and 10 are missing (and an absent peak is significant structurally), and peak 8 arises from complex arrangements very different from peak 7. Thus, even ignoring peak heights and profiles, the RCP bears little significant structural resemblance to the two dimensional packings, the coincidence at low distances being due to the necessary existence in a three dimensional packing of two dimensional arrangements of 2, 3 and 4 closely-spaced spheres. Looking at peak heights and profiles, we find an even greater divergence, reflecting the greater variability of local arrangements in the RCP case.

By examining the relevant geometrical ideas, we can also begin to understand the data of ref. 1 drawn from the rafts concerning solubility and Végard's law (we consider interstice sizes and variability), collineations (local sphere arrangements) and mechanical properties (out of plane movements of spheres); these again cause us to beware of extrapolating too much into three dimensions from two.

In summary, while wholeheartedly subscribing to the illustrative and qualitative values of two-dimensional models, we must not over-extrapolate into three dimensions, where the geometrical properties of space are different. Comparing two and three-dimensional models and packings is not only difficult but is of limited value. As a model of a very thin amorphous alloy (a few molecular diameters thick), however, the bubble raft model could have great specific predictive value.

Moreover, a close examination of the behaviour of a two-dimensional PDF when composition and/or radius ratio of the components are varied would be interesting in itself.

J. L. FINNEY*

Department of Physics,
University of Nevada, Reno,
Nevada 89503

Received August 8; revised November 10, 1972.

*Permanent address: Department of Crystallography, Birkbeck College, Malet Street, London WC1E 7HX.

- ¹ Simpson, A. W., and Hodgkinson, P. H., *Nature*, **237**, 230 (1972).
- ² Stanley, H. E., *Introduction to Phase Transitions and Critical Phenomena*, 17 (Oxford University Press, Oxford, 1971).
- ³ Polya, G., *Math. Ann.*, **84**, 149 (1921); Montroll, E. W., *Proc. Symp. Appl. Math., Ann. Math. Soc.*, **16**, 193 (1964); Montroll, E. W., and Weiss, G. H., *J. Math. Phys.*, **6**, 167 (1968).
- ⁴ Voronoi, G. F., *J. Reine Angew. Math.*, **134**, 198 (1908).
- ⁵ Finney, J. L., *Proc. Roy. Soc., A*, **319**, 479 (1970).
- ⁶ Smith, C. S., *Metal Interfaces*, 65 (Amer. Soc. Metals, Cleveland, 1965).
- ⁷ Bernal, J. D., *Proc. Roy. Soc., A*, **280**, 299 (1964).

BIOLOGICAL SCIENCES

Stimulation of Division of Density Inhibited Fibroblasts by Glucocorticoids

THE division of most "normal" cells in culture is inhibited after they have grown to a confluent monolayer. This is not the case with cells transformed either spontaneously or by tumour viruses¹. Alterations of the surface membrane might be partly responsible for this uncontrolled growth²: for example, protease action at the surface of contact-inhibited cells causes them to divide^{3,4} and acquire surface properties characteristic of malignant cells, such as increased agglutinability by plant lectins^{5,6}. Neuraminidase⁷ and hyaluronidase⁸ can also initiate division of contact-inhibited cells. Transformed cells contain abnormally large concentrations of proteases and glycosidases^{9,10} which are presumed to be lysosomal enzymes. Since cortisol stabilizes lysosomal membranes¹¹, we decided to investigate its effect on the growth of cells not subject to density inhibition of division: we found very little effect on 3T6, L, polyoma-transformed 3T3 (Py3T3) and SV40-transformed 3T3 (SV3T3) cells. However, cortisol unexpectedly stimulated DNA synthesis and division of density-inhibited 3T3 cells. This stimulation was specific for cells subject to contact inhibition of division. Density-inhibited early passage diploid human foreskin fibroblasts (HF cells) were similarly stimulated by cortisol. In addition, the stimulation was specific for steroids with high levels of glucocorticoid activity. The stimulation appears to involve changes in the cell surface, for treated 3T3 cells were agglutinated to a greater degree by concanavalin A than control cells.

Cells were grown as previously described¹² in Dulbecco-Vogt modified Eagle's medium containing 10% calf serum (3T3, Py3T3, and SV3T3 cells), 10% foetal calf serum (L cells), or 1.5–3% calf serum (HF cells, given by Dr David T. Kingsbury). We found no evidence of mycoplasma contamination of any cells following autoradiography with ³H-thymidine¹³. Neither control nor treated cells took up trypan blue or eosin Y dye, indicating that stimulation was not due to local areas of cell death. Agglutination by concanavalin A was determined as described before¹⁴.

The stimulation of DNA synthesis by confluent 3T3 cells showed a typical dose-response relationship (Fig. 1). The rate of synthesis was measured 23 h after addition of cortisol, the time of maximal stimulation. Addition of cortisol to final

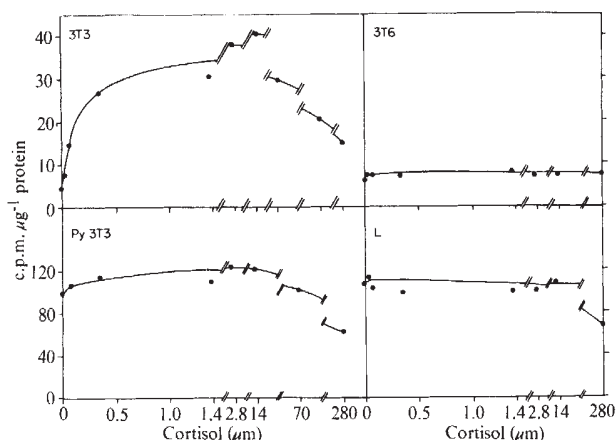


Fig. 1 Effect of cortisol on DNA synthesis by confluent 3T3, 3T6, Py3T3 and L cells. 1.5×10^4 cells were plated per 35 mm Falcon plastic tissue culture dish in 2 ml. of medium. After growth to confluency, cortisol (in ethanol) was added to the final designated concentration. The final ethanol concentration was usually 0.5% and never exceeded 1%, concentrations which had no effect on DNA synthesis or cell division. The cells were labelled for 15 min with ^3H -thymidine ($2.5 \mu\text{Ci/ml.}$; 0.4 Ci/mmol) 23 h after adding cortisol. They were then washed twice with cold phosphate-buffered saline, five times with cold 10% TCA, dissolved in 0.5 M KOH, and neutralized with HCl. Radioactivity was measured by liquid scintillation. Each point represents at least four determinations.

levels of $3.4 \times 10^{-7} \text{ M}$ to $2.8 \times 10^{-5} \text{ M}$ caused a six to nine-fold increase in the rate of thymidine incorporation into DNA. (The normal serum concentration of cortisol is about $3 \times 10^{-7} \text{ M}^{15}$, a concentration which produced a five-fold stimulation of thymidine incorporation.) Addition of $5 \times 10^{-6} \text{ M}$ cortisol to growing non-confluent 3T3 cells increased the rate of DNA synthesis only two-fold, even though it increased the final saturation density by 60%.

Stimulation was specific for contact-inhibited cells. As Fig. 1 shows, confluent cultures of Py3T3, 3T6 and L cells (from a C3H fibrosarcoma) responded only slightly to levels of cortisol which greatly stimulated 3T3 and HF cells. In addition, there was no significant stimulation or inhibition of DNA synthesis in non-confluent L and Py3T3 cells following cortisol treatment. The specificity was not simply a consequence of the very slow growth of confluent 3T3 and HF cells; slowly growing SV3T3 cells (in medium containing 0.15% serum) were as unresponsive as were growing SV3T3 cells.

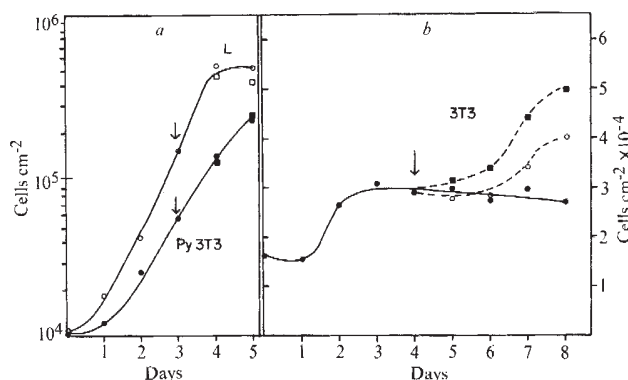


Fig. 2 Effect of cortisol on 3T3, Py3T3 and L cell number. Cells were plated at the indicated densities in 35 mm dishes. Cortisol was added to confluent cells at the times indicated by arrows. Cells were suspended with trypsin and counted in a haemocytometer. *a*: $\circ$, L cells; $\square$, L cells + $14 \mu\text{M}$ cortisol; $\bullet$, Py cells; $\blacksquare$, Py cells + $14 \mu\text{M}$ cortisol. *b*: $\bullet$, 3T3 cells; $\circ$, 3T3 cells + $0.07 \mu\text{M}$ cortisol; $\blacksquare$, 3T3 cells + $14 \mu\text{M}$ cortisol. Each point represents quadruplicate counts on duplicate plates.

Cortisol markedly stimulated division of contact-inhibited fibroblasts. Addition of cortisol to confluent 3T3 cells to $1.4 \times 10^{-5} \text{ M}$ produced an 85% increase in cell number (Fig. 2). The stimulatory action on contact-inhibited cells was confirmed with confluent cultures of the early passage human diploid fibroblasts; $2.8 \times 10^{-5} \text{ M}$ cortisol in the same conditions produced a 41% increase in cell number. Inclusion of $1 \times 10^{-5} \text{ M}$ $\text{N}^6, \text{O}^{2'}$ -dibutyryl cyclic AMP (dibutyryl cyclic AMP) with the cortisol did not reduce the stimulatory effect on confluent 3T3 cells. This same concentration of dibutyryl cyclic AMP suppresses almost totally the stimulation of cell division brought about by trypsin¹⁶. A higher concentration of dibutyryl cyclic AMP ($1 \times 10^{-3} \text{ M}$) partially reversed the cortisol stimulation of DNA synthesis and cell division.

The morphology of the 3T3 cells 4 days after addition of cortisol resembled that of transformed cells. They seemed to be layered on top of each other, and cell processes overlapped.

In contrast to the stimulation of division of contact-inhibited cells, no significant increase in cell number was detected when confluent L or Py3T3 cells were treated with cortisol (Fig. 2).

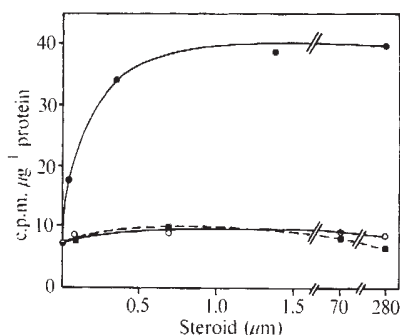


Fig. 3 Effect of cortisol, progesterone and cholesterol on DNA synthesis by confluent 3T3 cells. Culture and assay conditions were identical to those for 3T3 cells given in Fig. 1. (Each point is the average of duplicate determinations.) $\bullet$, Cortisol; $\circ$, cholesterol; $\blacksquare$, progesterone.

The stimulatory effect was specific not only to cell type but also to steroid. As Fig. 3 shows, broad concentrations of cholesterol and progesterone had virtually no effect on the rate of DNA synthesis 23 h after addition to confluent 3T3 cells. To rule out the possibility that a peak of DNA synthesis occurred at a time different from 23 h, we measured rates at 8 h intervals from 0–48 h. Neither cholesterol nor progesterone produced increases in thymidine incorporation at these times. In addition, no significant increase in cell number was detected by 60 h after addition of $1 \times 10^{-4} \text{ M}$ cholesterol or $2 \times 10^{-5} \text{ M}$ progesterone. The synthetic glucocorticoid, triamcinolone acetate, added at $2.5 \times 10^{-7} \text{ M}$, caused a 3.5-fold stimulation of DNA synthesis in confluent 3T3 cells 23 h after addition.

Agglutination of confluent 3T3 cells by 250 $\mu\text{g/ml.}$ concanavalin A was markedly increased over controls after 1 h of treatment with $1.4 \times 10^{-6} \text{ M}$ cortisol, suggesting not only that DNA synthesis and cell division might be initiated by a relatively brief treatment with cortisol, but also that the stimulatory action of cortisol might result from changes it elicits in the surface membrane. Induction of tyrosine amino-transferase in hepatoma cells by cortisol correlates with altered surface properties¹⁷, and increased agglutination by wheat germ lipase agglutinin has been demonstrated after glucocorticoid treatment of HeLa cells¹⁸.

Cortisol has diverse effects on mammalian cells: it is cytotoxic to lymphoid cells¹⁹, inhibits DNA synthesis and division of L929 cells²⁰, and leads to the induction in hepatic and hepatoma cells of enzymes involved in amino-acid catabolism^{17,21}. Specific cytoplasmic glucocorticoid receptors have been demonstrated in lymphoma²², L929²³ and hepatoma²⁴ cells. Glucocorticoid-resistant cell lines have been

derived from the lymphoma²² and L929²³ cell lines, and the resistance seems to be due to decreased cytoplasmic glucocorticoid binding capability. We are investigating the possibility that transformation of 3T3 cells by tumour viruses decreases the glucocorticoid binding capability of the cells.

Several observations suggest that cortisol stimulates division of contact-inhibited cells by altering acid mucopolysaccharide metabolism. (a) Cortisol inhibits the synthesis of acid mucopolysaccharides in skin and connective tissue²⁴. (b) The intrinsic viscosity of hyaluronic acid secreted by human synovial cells is reduced by cortisol treatment and during rapid cell growth²⁵. (c) Treatment of density-inhibited secondary mouse embryo cells with hyaluronidase causes cell division⁸. (d) Transformation of 3T3 cells by polyoma or SV40 viruses causes decreased synthesis of hyaluronic acid²⁷. This system should lend itself to biochemical analysis to test the above as well as alternative mechanisms.

This work was supported by a grant from the National Cancer Institute of the US Public Health Service. C. R. T. was supported by a US Public Health Service training grant to the Department of Molecular Biology and Biochemistry. We thank Mr Tom Ho for technical help.

CORNELIA R. THRASH
D. D. CUNNINGHAM

Department of Medical Microbiology,
California College of Medicine,
University of California, Irvine 92664

Received November 12, 1972.

- ¹ Todaro, G. J., Green, H., and Goldberg, B., *Proc. US Nat. Acad. Sci.*, **51**, 66 (1964).
- ² Burger, M. M., *Current Topics in Cellular Regulation*, **3**, 135 (1971).
- ³ Burger, M. M., *Nature*, **227**, 170 (1970).
- ⁴ Sefton, B. M., and Rubin, H., *Nature*, **227**, 843 (1970).
- ⁵ Burger, M. M., *Proc. US Nat. Acad. Sci.*, **62**, 994 (1969).
- ⁶ Inbar, M., and Sachs, L., *Proc. US Nat. Acad. Sci.*, **63**, 1418 (1969).
- ⁷ Vaheri, A., Ruoslahti, E., and Nordling, S., *Nature New Biology*, **238**, 211 (1972).
- ⁸ Vasiliev, J. M., Gelfand, I. M., Guelstein, V. I., and Fetisova, E. K., *J. Cell. Physiol.*, **75**, 305 (1970).
- ⁹ Bosmann, H. B., *Exp. Cell. Res.*, **54**, 217 (1969).
- ¹⁰ Bosmann, H. B., *Biochim. Biophys. Acta*, **264**, 339 (1972).
- ¹¹ Weissman, G., and Thomas, L., *Rec. Prog. Hormone Res.*, **20**, 215 (1964).
- ¹² Cunningham, D. D., *J. Biol. Chem.*, **247**, 2464 (1972).
- ¹³ Nordone, R. M., Todd, J., Gonzales, P., and Gaffney, E. V., *Science*, **149**, 1100 (1965).
- ¹⁴ Ozanne, B., and Sambrook, J., *The Biology of Oncogenic Viruses* (edit. by Silvestri, L. G.) (North-Holland, Amsterdam, 1971).
- ¹⁵ Cantarow, A., and Trumper, M., in *Clinical Biochemistry* (Saunders, Philadelphia, 1962).
- ¹⁶ Burger, M. M., Bombik, B. M., Breckenridge, B. M., and Sheppard, J. R., *Nature New Biology*, **239**, 161 (1972).
- ¹⁷ Ballard, P. L., and Tomkins, G. M., *Nature*, **224**, 344 (1969).
- ¹⁸ Salvato, J., and Melnykovich, G., *In Vitro*, **7**, 247 (1972).
- ¹⁹ Dougherty, T. E., and White, A., *Amer. J. Anat.*, **77**, 81 (1945).
- ²⁰ Pratt, W. B., and Aronow, L., *J. Biol. Chem.*, **241**, 5244 (1966).
- ²¹ Schimke, R. T., Sweeney, E. W., and Berlin, C. M., *J. Biol. Chem.*, **240**, 322 (1965).
- ²² Rosenau, W., Baxter, J. D., Rousseau, G. G., and Tomkins, G. M., *Nature New Biology*, **237**, 20 (1972).
- ²³ Pratt, W. B., and Ishii, D. N., *Biochemistry*, **11**, 1401 (1972).
- ²⁴ Baxter, J. D., and Tomkins, G. M., *Proc. US Nat. Acad. Sci.*, **68**, 932 (1971).
- ²⁵ Schiller, S., and Dorfman, A., *Endocrinology*, **60**, 376 (1957).
- ²⁶ Cantor, C. W., and Prince, R. K., *Biochim. Biophys. Acta*, **83**, 165 (1964).
- ²⁷ Hamerman, D., Todaro, G. J., and Green, H., *Biochim. Biophys. Acta*, **101**, 343 (1965).

Preliminary Evidence for Cholinceptive Sites in the Excitability of Spermatozoa

THE spermatozoa of a variety of species respond to mechanical stimuli and to other physical and chemical alterations in the environment by changing speed and pattern of movement,

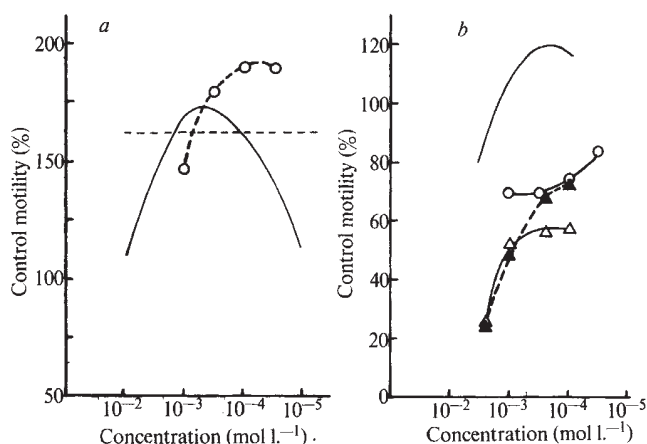


Fig. 1 Sea urchin sperm motility. *a*, Sperm cells suspended in filtered seawater containing 20 mM dimethyl sulphoxide. Solid line without data points, redrawn from ref. 2. Open circles, effect of adding varying concentrations of d-tubocurarine (curare) to sperm suspension containing 100 μ M acetylcholine. Dashed line indicates swimming rate of sea urchin sperm in 100 μ M acetylcholine and DMSO without curare. *b*, Sperm cells suspended in filtered seawater containing 20 mM dimethyl sulphoxide (except for solid line without data points, redrawn from ref. 2, in which no DMSO was present). Open circles, effect of varying concentrations of curare on sperm swimming speed in filtered seawater containing 20 mM DMSO. Open triangles, effect of adding varying amounts of acetylcholine to sperm exposed to 100 μ M curare; and closed triangles, sperm exposed to 1 mM curare before adding acetylcholine.

amplitude, frequency and length of the flagellar wave, as well as membrane potential¹⁻⁴. The propagation of the flagellar wave thus cannot be attributed entirely to an autonomous oscillator, but must be influenced in a "neurochemical" fashion analogous to smooth muscle control systems.

Spermatozoa from mammals, fishes, echinoderms and molluscs possess highly active cholinesterases⁵; the swimming speed of mussel, sea urchin and starfish sperm cells changes biphasically and in dose-dependent fashion in eserine and diisopropylfluorophosphate as well as in acetylcholine and curare^{1,2}. Motility is conveniently measured by the centrifuge-orientation, absorbance change method². Acetylcholine and curare^{1,2} both increase and decrease the velocity of sea urchin sperm by about 20% or less depending on the concentration. Dimethyl sulphoxide, which at a concentration of 20 mmol l⁻¹ does not visibly affect sperm motility but facilitates permeation by the pharmacological agents, permits 1 mM acetylcholine to cause an initial increase in sperm movement to 170% of the control rate². Curare, when added with DMSO, causes a 25-30% reduction in motility in the same concentration range that maximally increased the swim rate² in the absence of DMSO (Fig. 1*a* and *b*).

Examination of the effects of the interaction of these two drugs reveals that, in DMSO, 1 mM acetylcholine (which optimally increased the sperm cell velocity) now, in combination with curare, doubles the inhibitory effect of the curare alone (Fig. 1*b*). Raising the acetylcholine concentration to 4 mmol l⁻¹, while holding the curare at 100 μ M, further depresses the swimming speed to 25% of the control rate. Acetylcholine in equimolar concentrations with the curare (100 μ mol l⁻¹) acts neither synergistically nor antagonistically on the curarized spermatozoa. On the other hand, addition of curare to sperm cells pretreated with 100 μ M acetylcholine again results in a biphasic, dose-dependent response (Fig. 1*a*). Curare in ten-fold excess of the acetylcholine (curare/ACh = 1 mmol/100 μ mol) reduces the sperm speed to 150% of the control from the 165% without curare, while at a 5 to 1 ratio the swim rate goes up to 180% of the control. With both at equimolar concentrations and at a 1 to 2 ratio (curare/ACh = 50 μ mol/100 μ mol) the swim speed now increases to 190% of the control

rate. The order of the drug administration is apparently quite consequential.

Decamethonium, another pharmacologic agent which reportedly acts on the receptor sites at synapses and neuromuscular junctions in vertebrates, also affects the motility of sea urchin sperm cells. Concentrations of decamethonium in seawater above $10\ \mu\text{M}$ depress the swimming speed, while at $10\ \mu\text{M}$ or below there is a slight increase above the control swim rate. In the vertebrate system depolarization induced by decamethonium appears initially to stimulate then block impulse transmission⁶. The sperm cells' response may be regarded as comparable; presumably irreversible depolarization occurring more rapidly at the higher concentrations and requiring longer exposure at the lower concentrations, the stimulatory effect of the low concentration of decamethonium persists long enough to be detected by my method.

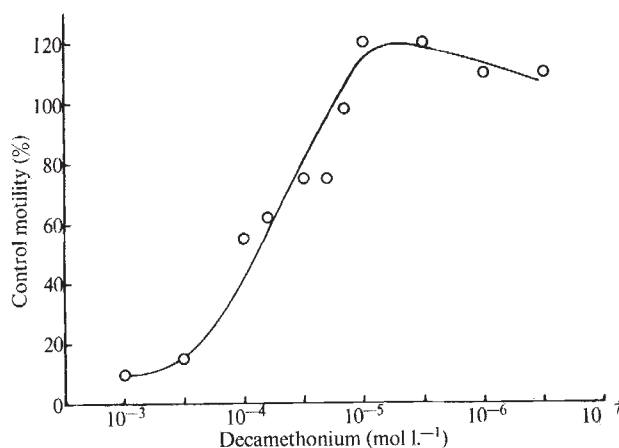


Fig. 2 Sperm cells suspended in filtered seawater. Effects of varying concentrations of decamethonium.

These preliminary observations appear to lend support to the thesis that an intracellular control system, cholinergic in nature, imposes a modulatory effect on the purely contractile components which underlie flagellar wave propagation². Moreover, this cellular "governor", under appropriate circumstances, may serve to mediate the effects of some extraneous factors on sperm motility. If decamethonium's primary pharmacologic action does indeed depend on its depolarization effects, the data presented here are further consistent with our interpretation that changes in the membrane potential bear a causal relationship to the sperm cell's motile performance³. Dryl's description⁷ of the role of the cell membrane of ciliates in both the reception of external stimuli and in stimulus conduction may have some bearing relative to the nature of spermatozoan response mechanisms.

This work has been supported by a grant from the National Institute of Child Health and Human Development.

LEONARD NELSON

Department of Physiology,
Medical College of Ohio,
Toledo, Ohio 43614

Received November 16, 1972.

¹ Nelson, L., *Biol. Reprod.*, **6**, 319 (1972).

² Nelson, L., *Exp. Cell Res.*, **74**, 269 (1972).

³ Gaddum, P., *Anat. Record*, **161**, 471 (1968).

⁴ McGrady, A. V., and Nelson, L., *Exp. Cell Res.*, **73**, 192 (1972).

⁵ Mann, T., in *Biochemistry of Semen and the Male Reproductive Tract*, 2nd ed., 209 (Methuen, London, 1964).

⁶ Koelle, G. B., in *The Pharmacological Basis of Therapeutics* (edit. by Goodman, L. S., and Gilman, A.), 4th ed., 601 (Macmillan, New York, 1970).

⁷ Dryl, S., *Acta Protozool.*, **7**, 325 (1970).

Headspace Volatiles of Marijuana

THE sweet odour of *Cannabis sativa*, L. is of special interest as a possible means for the detection of illicit marijuana traffic. US Customs dogs, trained to alert to the odour of marijuana and hashish, are successfully used to find concealed contraband. An electronic "sniffer" that employs a portable quadrupole mass spectrometer to detect volatile vapours of drugs, including marijuana, has also been demonstrated¹. Although the chemical composition of the essential oil of marijuana has been thoroughly investigated²⁻⁴, little is known about the specific composition of the emitted aroma constituents. The composition of this headspace, as it contributes directly to the odour, is more significant than that of the oil for the characterization of marijuana aroma⁵. Aroma profiles of fruits and vegetables have been obtained by direct gas chromatographic analysis of the headspace components⁶. We used this technique, which permits rapid determination of labile constituents under mild analytical conditions, to analyse marijuana aroma, and we report our preliminary results here.

An authentic marijuana standard containing 1.9% Δ^9 -THC and grown from Mexican seed was obtained (National Institute of Mental Health programme, University of Mississippi School of Pharmacy). Additional samples of marijuana ranging in Δ^9 -THC content from 1.4% to 3.1% were obtained from Customs' seizures. All samples were stored at -25°C . Authentic samples of mono- and sesqui-terpenes and other compounds detected in the headspace were obtained from commercial sources or donated.

Marijuana was prepared routinely for vapour sampling by placing 1 g in a microvial fitted with an on-off valve and septum, and after equilibration for 1 h at 65°C , 5 ml. of the headspace air containing the volatiles was withdrawn using a gastight syringe and immediately injected into a gas chromatograph. The aroma profiles obtained included compounds present in the headspace at concentrations as low as $0.1\ \text{ng ml}^{-1}$, which is below the human odour threshold for many odorants⁷. A 'Perkin-Elmer 900' and a 'Hewlett Packard 7610A' chromatograph, equipped with flame ionization detectors and interfaced with a 'Perkin-Elmer PEP-1' gas chromatography data system, were used. Chromatography was carried out using six-foot glass columns (2 mm i.d.) packed with 3% 'OV-101' on 100/120 'Gas Chrom Q' or 20% 'Reoplex 400' on 80/100 'Chromosorb W', acid washed, with helium as carrier gas ($50\ \text{ml. min}^{-1}$). The columns were programmed from 35°C ('OV-101') or 60°C ('Reoplex 400') to 130°C at 6°C min^{-1} to obtain complete headspace chromatograms. A third column packed with 'Chromosorb 101', 80/100 was operated at 90°C to separate certain of the low-boiling components.

Typical headspace chromatograms show three separate fractions based on ascending order of component boiling points: Fraction I consists of oxygenated compounds ($\text{MW} < 100$); Fraction II consists of mono-terpene hydrocarbons and oxygenated compounds ($\text{MW} > 100$); Fraction III consists of sesqui-terpene hydrocarbons ($\text{MW} > 200$). Separated constituents were identified by comparison of their relative retention times (RRT) obtained on the adsorption columns of different polarity with the RRT of authentic standards, and by their mass spectra obtained in separate experiments using a 'Finnigan 3000 GC-MS' system with computer data acquisition. Examination of Fraction I using the 'Chromosorb 101' column confirmed the presence of acetone (approximately 75% of this fraction) and smaller amounts of methanol, acetaldehyde, ethanol, methyl acetate and iso-butyraldehyde. As these compounds are commonly found in most plant vapours and are not unique, Fraction I, comprising approximately 10% of the total headspace, was not analysed further.

The components found in Fractions II and III and their RRT as separated on the two columns are listed in Table 1. It is of interest to note that 2-methyl-2-heptene-6-one has not been previously reported as a constituent of marijuana. The relative percentage composition of the headspace components

Table 1 Relative Retention Times of Components of Marijuana Headspace

Component	3% OV 101 32° C (ref. limonene)	3% OV 101 90° C (ref. β -caryophyllene)	20% Reo- plex 80° C (ref. limonene)	20% Reo- plex 120° C (ref. β -caryophyllene)
α -Pinene	0.46		0.37	
Camphene	0.51		0.49	
β -Pinene	0.63		0.60	
2-Methyl-2-heptene-6-one	0.62			0.32
Myrcene	0.77		0.78	
Δ^3 -Carene	0.86		0.69	
α -Terpinene	0.90		0.89	
Limonene	1.00		1.00	
β -Phellandrene	1.00		1.07	
cis-Ocimene	1.14		1.19	
trans-Ocimene	1.24		1.32	
γ -Terpinene	1.28		1.29	
Terpinolene	1.63		1.59	
Linalool	1.84			0.73
β -Caryophyllene		1.00		1.00
trans- α -Bergamotene		1.12		0.87
β -Farnesene		1.26		1.23
Humulene		1.19		1.39

was determined by integration of the area under each peak appearing in the chromatograms (Fractions II and III only), and a typical analysis is shown in Table 2. Data showing the composition of the oil of marijuana obtained by analysis of a steam distillate of the standard sample are included for comparison. Fenchyl alcohol, borneol, β -bisabolene and 2-methyl-2-heptene-6-one are components not previously reported in the oil⁴. As indicated in Table 2, quantitative results accounted for 98.3% of the headspace components of Fractions II and III and 96% of the oil. The remaining 1.7% of the headspace consists of one unidentified mono-terpene hydrocarbon; the remaining 4% of the oil is composed of unidentified mono-terpene alcohols (1%) and sesqui-terpene alcohols (3%).

Numerous seizure samples of Mexican marijuana were examined and similar headspace profiles were obtained. It is apparent from the data recorded in Table 2 that the com-

Table 2 Comparison of Headspace Composition of Marijuana with Composition of Essential Oil

Component	Headspace Composition (%)	Essential oil Composition (%)
α -Pinene	55.5	3.9
Camphene	0.9	0.7
β -Pinene	16.4	2.2
2-Methyl-2-heptene-6-one*	0.4	0.6
Δ^3 -Carene	0.6	0.1
Myrcene	8.3	1.0
α -Terpinene	t†	t
Limonene	5.4	1.0
β -Phellandrene	1.2	0.2
cis-Ocimene	3.2	0.7
trans-Ocimene	t	t
γ -Terpinene	0.8	0.6
Terpinolene	—	0.1
p-Cymene	t	0.5
Linalool	—	0.1
Fenchyl alcohol*	—	t
Borneol*	0.7	8.0
trans- α -Bergamotene	3.4	37.5
β -Caryophyllene	0.8	9.8
β -Farnesene	—	1.0
α -Terpinenol	0.7	13.9
β -Humulene	—	2.2
α -Selinene	—	3.2
β -Bisabolene*	—	1.4
Curcumene	—	7.4
Caryophyllene oxide	98.3	96.0
Total identified components		

* Not previously reported as constituent of marijuana.

† t, trace, <0.1%.

position of the headspace differs markedly from that of the oil, as fewer components are present in different relative proportions. The principal constituents comprising about 85% of the headspace (α -pinene, β -pinene, myrcene, limonene) represent less than 10% of the oil. The minor contribution of oxygenated terpenes to the headspace is shown by the fact that fenchyl alcohol, borneol, and α -terpinenol are all present in the oil but none was detected in the headspace. These results can be explained largely on the basis of the lower volatility of the alcohols. The greater affinity of the oxygenated species for the plant medium, however, *vis à vis* the terpene hydrocarbons is also a significant factor. The compound p-cymene found in the oil but not in the headspace may be an artefact formed during preparation of the oil⁸.

The present unique analysis of the headspace composition is of fundamental value in the characterization of the aroma of marijuana as it reflects the relative volatility of the plant constituents and the complex manner through which these constituents are bound in the plant. The examination of the headspace of marijuana samples of different geographic origin is under study. These data will thus provide the basis for an investigation of the complex biochemical mechanism involved in the detection of marijuana by detector dogs.

We thank V. Venturella for help in examination of Fraction I, C. Erikson for preparation of the oil of marijuana and Givaudan Corp., Clifton, NJ., Fritsche D and O, Inc., New York, and Hercules, Inc., Wilmington, Delaware, for providing samples of headspace compounds.

L. V. S. HOOD
M. E. DAMES
G. T. BARRY

Division of Technical Services,
US Bureau of Customs,
Washington DC

Received October 16, 1972.

- Green, D. E., *Intra-Science Chemistry Reports*, **4**, 211 (1970).
- Martin, L., Smith, D. M., and Farmilo, C. G., *Nature*, **191**, 774 (1961).
- Nigam, M. C., Handa, K. L., Nigam, I. C., and Levi, L., *Can. J. Chem.*, **43**, 3372 (1965).
- Bercht, C. A. L., Kuppers, F. J. R. M., Lousberg, R. J. J. Ch., Saleminck, C. A., Svendsen, A. B., and Karlsen, J., *UN Secretariat*, No. 29, July 22 (1971).
- Nawar, W. W., *J. Agr. Food Chem.*, **19**, 1057 (1971).
- Buttery, R. G., and Teranishi, R., *Anal. Chem.*, **33**, 1439 (1961).
- Dravnick, A., and O'Donnell, A., *J. Agr. Food Chem.*, **19**, 1049 (1971).
- Guenther, E., and Althausen, D., *The Essential Oils* (Van Nostrand, New York, 1949).

Effects of Marijuana and Tobacco Smoke on DNA and Chromosomal Complement in Human Lung Explants

HUMAN lung explants exposed to smoke from marijuana or from Kentucky Standard tobacco cigarettes have been reported to display abnormalities of cell morphology, mitosis, DNA synthesis and atypical proliferation¹. We report here a study designed to test the effects of both types of smoke on the DNA and chromosomal complement.

We used the model system for exposing lung explants to puffs of fresh smoke in standardized conditions². Lung explants were prepared and exposed to cigarettes made from marijuana or Kentucky Standard tobacco as previously described¹. Normal lung tissue was obtained not only from older patients operated on for pulmonary tumours¹, but also from a healthy young man (age 25 yr) killed in an accident. The DNA determinations were carried out in metaphases and telophases of the original stained lung cultures by Feulgen microfluorometry³. All together, over 2,000 cells were examined in control cultures and correspond-

ing cultures from 4–70 days after exposure to marijuana or tobacco cigarette smoke. Chromosomes were prepared according to Inui *et al.*³ for the examination of chromosomal status.

Results were reproducible in all cultures. They were essentially the same for cultures derived from the lungs of the older tumour patients and from those derived from the healthy young man. There was a good agreement between chromosomal and DNA data.

Table 1 Comparison between the DNA Content* in Metaphases† and Telophases‡, and Number of Chromosomes§ in Fibroblastic Cells of a Control Adult Human Lung Explant and after Exposure to Fresh Smoke from Marijuana and Kentucky Standard Cigarettes

Type of experiment	Mean frequency in per cent					
	DNA content in metaphases		DNA content in telophases		Number of chromosomes	
	4 DNA	≈ 4 DNA	2 DNA	≈ 2 DNA	2 N	≈ 2 N
Control	76	24	70	30	56	44
Kentucky Standard	56	44	48	52	36	64
	<i>p</i> Co=0.01		<i>p</i> Co<0.0065		<i>p</i> Co<0.0005	
Marijuana	52	48	52	48	31	69
	<i>p</i> Co=0.0025		<i>p</i> Co<0.005		<i>p</i> Co<0.0005	

* Microfluorometry.

† 431 cells measured.

‡ 633 metaphases counted.

There were significant differences between control and exposed cultures; 4–28 days after exposure to marijuana or Kentucky tobacco cigarettes there was not only a significant decrease in number of cells with 2N, and with 4 DNA and 2 DNA (Table 1), but the variability of cells with deviating DNA content and chromosomal numbers was greater than that observed in controls (Figs. 1 and 2). This difference was especially marked after marijuana cigarette smoke.

It thus seems that exposure of human lung explants to fresh smoke from marijuana or Kentucky Standard tobacco cigarettes evokes not only abnormalities in DNA synthesis, mitosis and growth¹, but also results in alterations of DNA and chromosomal complement, that is in a disturbance of the genetic equilibrium of the cell population. The finding that these changes were

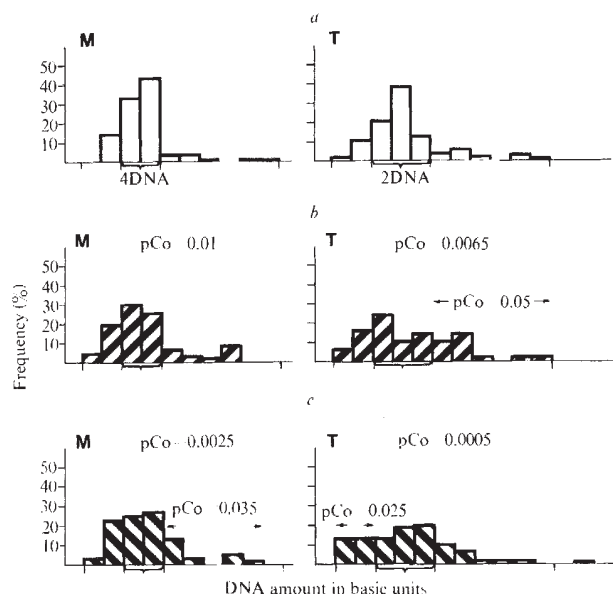


Fig. 1 Comparison between the DNA content (Feulgen microfluorometry) in metaphases (M) and telophases (T) of fibroblastic cells (number measured = 431) from (a) control adult human lung explant, and after exposure to fresh smoke from (c) marijuana, and (b) Kentucky Standard cigarettes (5 experiments).

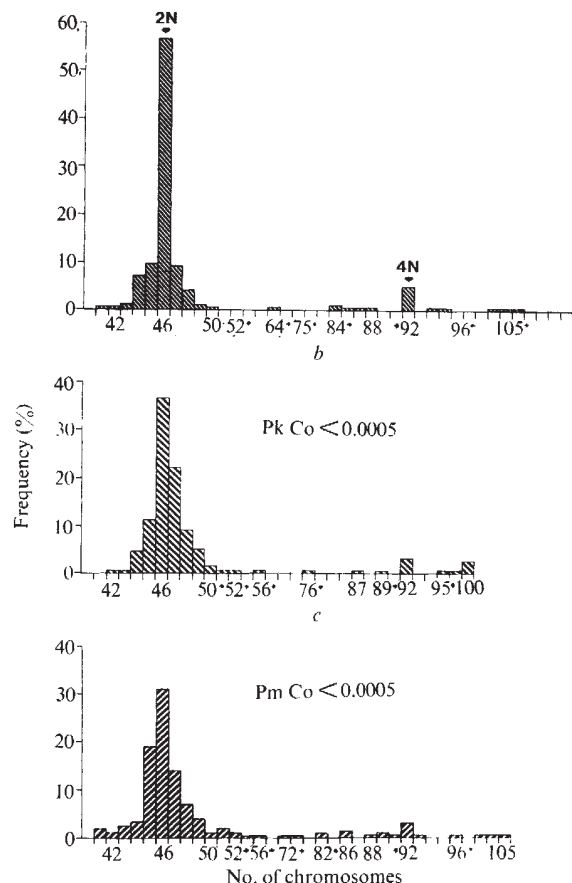


Fig. 2 Comparison between the number of chromosomes of fibroblastic cells (number of metaphases counted = 633) from (a) control adult human lung explant and after exposure to fresh smoke from (c) marijuana and (b) Kentucky Standard cigarettes (number of cultures examined = 12).

observed very early, and that they persisted for prolonged periods after exposure, indicates that these alterations are not lethal to the cells. The question whether the cells with abnormal DNA and chromosomal complement are responsible for the subsequent atypical growth, and may represent an early stage preceding malignant transformation, cannot be answered at present.

We thank Drs J.-P. Mach and G. Pusztaszeri for normal lung tissue, and Mrs N. Friderici for technical assistance. This work was supported in part by grants from WHO and ASFC.

C. LEUCHTENBERGER
R. LEUCHTENBERGER
U. RITTER

Swiss Institute for
Experimental Cancer Research,
Lausanne

N. INUI

Department of Experimental Pathology,
Cancer Institute,
Tokyo

Received November 13, 1972.

¹ Leuchtenberger, C., Leuchtenberger, R., and Schneider, A., *Nature*, **241**, 137 (1973).

² Leuchtenberger, C., and Leuchtenberger, R., *Exp. Cell. Res.*, **62**, 161 (1970).

³ Inui, N., Takayama, S., and Sugimura, T., *J. Nat. Canc. Inst.*, **48**, 1409 (1972).

Specific Biosynthesis of an Envelope Protein of *Escherichia coli*

A UNIQUE structural protein in the envelope of *Escherichia coli*, of molecular weight about 7,500 and covalently attached to the peptidoglycan, was found by Braun *et al.*¹⁻⁴, who reported that its amino-acid sequence lacks histidine, proline, glycine, cysteine, phenylalanine and tryptophan⁴. Recently we found that this lipoprotein exists in the *E. coli* envelope fraction not only in the bound form found by Braun *et al.*¹⁻³ but also in a free form, not covalently attached to the peptidoglycan⁵. This form accounts for about two-thirds of the lipoprotein and serves as a precursor of the bound form⁵. We describe here an *in vivo* situation in which only this lipoprotein is biosynthesized. Using these conditions, we have investigated the biosynthetic mechanism of this envelope protein.

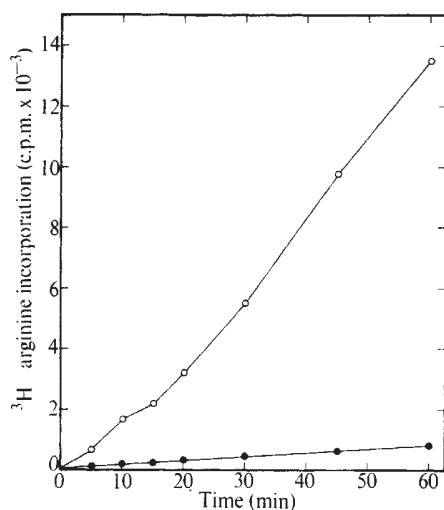


Fig. 1 ^3H -arginine incorporation into hot TCA-insoluble fraction in the presence or absence of histidine. *E. coli* CP78 ($\text{RC}^+\text{his}^-\text{leu}^-\text{thr}^-\text{arg}^-\text{B}_1^-$)⁶ was grown at 41°C in M9 medium containing all necessary supplements (20 $\mu\text{g}/\text{ml}$. of each amino-acid and 2 $\mu\text{g}/\text{ml}$. of thiamine). At about 2×10^8 cells/ml., 20 ml. of the culture was filtered with an HA 'Millipore' filter and washed with M9 medium lacking histidine. Then the cells were resuspended in 20 ml. of M9 medium containing all necessary supplements except histidine. To 1 ml. of the suspension was added 2 μCi of ^3H -arginine (without histidine) and to another 1 ml. of the suspended cells 2 μCi of ^3H -arginine and 20 μg of histidine (+histidine). They were then incubated at 41°C . At the time intervals indicated in the figure, 0.1 ml. of the cell suspension was applied to a filter paper disk (Whatman 3 MM, 2.4 cm diameter). Immediately after the sample was spread throughout the disk, it was put into chilled 5% TCA solution containing 100 $\mu\text{g}/\text{ml}$. unlabelled arginine and kept standing in the ice bath for 60 min. These disks were then boiled for 30 min in a 5% TCA solution. After cooling, they were washed three times by changing the TCA solution, soaked in acetone, and dried. Radioactivity was determined in a liquid scintillation counter. ○, With histidine; ●, without histidine.

We have investigated whether this lipoprotein is biosynthesized in cells starved for one of the amino-acids which it lacks⁴. We measured ^3H -arginine incorporation into the hot trichloroacetic acid (TCA) insoluble fraction in the presence and absence of histidine using a histidine auxotroph, *E. coli* CP78⁶($\text{RC}^+\text{his}^-\text{leu}^-\text{thr}^-\text{arg}^-\text{B}_1^-$; given by Dr J. Gallant). As Fig. 1 shows, about 95% of total incorporation was inhibited by histidine starvation. Although the rate of incorporation was strikingly reduced in the absence of histidine, ^3H -arginine continued to be incorporated into the protein fraction at a low but constant rate. This suggests that some proteins lacking histidine, such as the lipoprotein described here, are produced preferentially during the histidine starvation. We

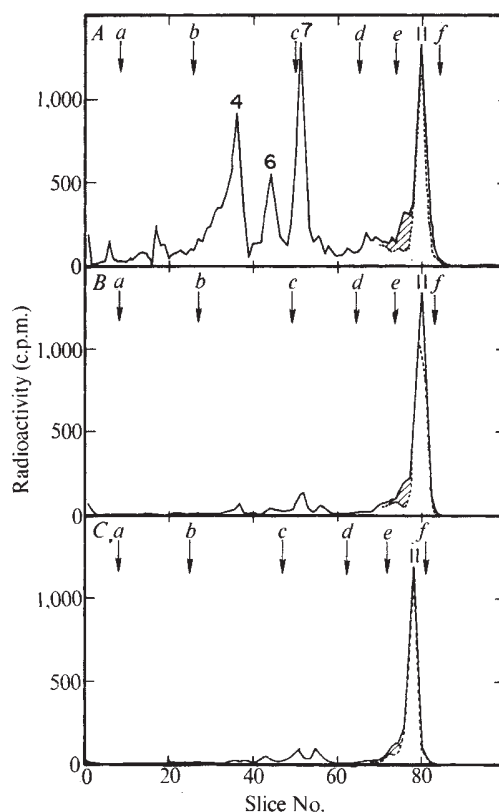


Fig. 2 SDS polyacrylamide gel electrophoresis of the envelope fractions labelled with ^{14}C -arginine in the presence and absence of histidine. Fifty ml. of an exponential culture (about 4×10^8 cells/ml.) of *E. coli* CP78 was filtered and the cells were resuspended in 50 ml. of M9 medium lacking histidine. **A**, Control: 10 ml. of the suspension was diluted twice with M9 medium. ^{14}C -arginine (5 μCi) and 20 $\mu\text{g}/\text{ml}$. histidine (final concentration) was added to the culture, and it was incubated for 1 h at 41°C . The turbidity increased from 53 to 106 Klett units during incubation. **B**, Without histidine (0~1 h): 20 ml. of the suspension was added 5 μCi of ^{14}C -arginine and the mixture was incubated for 1 h at 41°C . The turbidity increased from 106 to 111 Klett units during the incubation. **C**, Without histidine (2~4 h): 20 ml. of the suspension was incubated for 2 h at 41°C . The 5 μCi of ^{14}C -arginine was added to the culture and it was incubated for another 2 h. The turbidity increased from 106 to 118 Klett units during the whole incubation period (4 h). Envelope fractions were prepared by differential centrifugation as previously described⁷. Half of each envelope fraction was treated with T4 phage lysozyme to analyse the bound form of the lipoprotein as described previously⁵. Both fractions (T4 phage lysozyme treated and untreated) were then solubilized and subjected to SDS gel electrophoresis (7.5% acrylamide gels) in separate gels. Gel electrophoresis was carried out with DANS-internal standards of molecular weights⁸. The patterns of the envelope fractions with and without the lysozyme treatment were superimposed with the aid of the internal standards. —, Envelope fractions treated with T4 phage lysozyme; - - -, without T4 phage treatment. In the case of the fraction without the treatment, only the part between e and f is shown. Positions of internal standards of molecular weight are shown by small arrows: a, dimer; b, monomer of DANS-bovine serum albumin; c, dimer; d, monomer of DANS-egg white lysozyme; e, cytochrome c; f, DANS-insulin. Peak numbers correspond to those previously reported⁷.

therefore analysed envelope proteins synthesized in the presence of ^{14}C -arginine but in the absence of histidine by sodium dodecyl sulphate gel electrophoresis. As Fig. 2B and C show, ^{14}C -arginine was almost exclusively incorporated into peak 11 in the absence of histidine, in contrast to the situation in the presence of histidine (Fig. 2A). There was substantial incorporation of ^{14}C -arginine into peak 11 not only during the first hour but also between 2 and 4 h after removal of histidine (Fig. 2B and C).

We have shown previously that the free form of the lipoprotein forms peak 11 and the bound form gives rise to a

shoulder at the higher molecular weight side of peak 11 after lysozyme treatment of the envelope fraction⁵. Fig. 2 also shows the analysis of the bound form (shaded in Fig. 2), which decreased considerably relative to the amount of the free form (peak 11) as the time of histidine starvation increased. This is understandable if the enzymes responsible for the conversion of the free form to the bound form are not biosynthesized in the absence of histidine, and probably reduced in amount due to protein turnover. We also analysed the soluble proteins labelled with ¹⁴C-arginine in the absence of histidine. The incorporation of ¹⁴C-arginine into soluble proteins was inhibited considerably by histidine starvation in all fractions separated by SDS gel electrophoresis (data not shown).

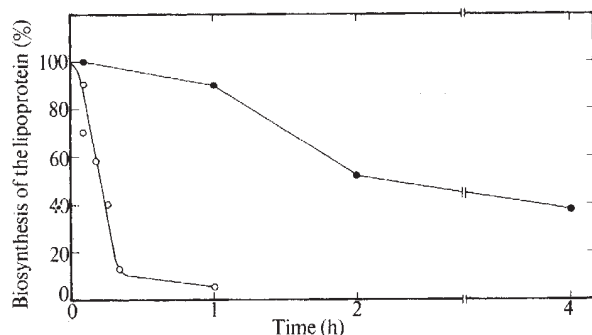


Fig. 3 Stability of biosynthesis of the lipoprotein. *E. coli* CP78 was starved of histidine as described in Fig. 2. To one of the starved culture was added 200 µg/ml. of rifampicin at zero time (○), and the other culture was incubated without the drug (●). At time intervals indicated in the figure, 15 ml. of the culture was taken, and 25 µCi of ³H-arginine was added to it. The mixture was incubated for 5 min at 41° C, and chilled rapidly. The amount of the lipoprotein produced was determined as described in Fig. 2, and expressed as percentage of the amount of the protein produced in the presence of histidine between 5 and 10 min.

We have the following evidence that the protein at peak 11 produced in the absence of histidine is not due to smaller pieces of proteins of higher molecular weights⁹, but is the same protein produced in the presence of histidine, that is, the lipoprotein mentioned above. (1) The unique ratio of arginine to tyrosine content of peak 11⁵ was not affected by histidine starvation. (2) When the cells were starved for amino-acids such as leucine, threonine and arginine which are components of the lipoprotein, synthesis of peak 11 protein was greatly reduced, in contrast to the situation in cells starved of histidine (Table 1). (3) The exclusive biosynthesis of the peak 11 protein in the absence of histidine was also observed in another histidine auxotroph, *E. coli* 4220 (his⁻; given by Dr M. Riley). (4) The same exclusive production of the peak 11 protein was observed with a tryptophan and proline auxotroph, *E. coli* X7165 (trp⁻ pro⁻; given by Dr K. Ihler).

To examine whether the protein is biosynthesized *de novo* on ribosomes, we tested the effects of well known inhibitors of protein synthesis on the production of the peak 11 protein in the absence of histidine, using *E. coli* CP78 (Table 2). Tetracycline and chloramphenicol were highly inhibitory to the lipoprotein synthesis; judging from their specificities¹⁰, this indicates that the lipoprotein is synthesized on ribosomes *de novo*. In contrast to tetracycline and chloramphenicol, rifampicin had little effect on the biosynthesis of the lipoprotein, suggesting that mRNA for the protein is extraordinarily stable, since the present concentration of the drug used (200 µg/ml.) almost completely blocked RNA synthesis (Table 2).

Stability of mRNA for the protein was examined by measuring biosynthetic rates of the protein after the addition of rifampicin to the culture of *E. coli* CP78 starved of histidine (Fig. 3). It was found that mRNA for the protein is extra-

ordinarily stable with a half life of about 12 min in comparison with those of mRNA of other *E. coli* proteins¹¹⁻¹³. Fig. 3 also shows changes in the biosynthetic rates of the protein after histidine starvation in the absence of rifampicin. After 1 h of starvation the protein was still almost fully synthesized, and even after 4 h, 30% of the full activity was maintained (Fig. 3). Since the half life of the mRNA is 12 min, one can conclude that mRNA for the protein is continuously synthesized for as long as 4 h in the cells starved of histidine. Similar synthesis of mRNA in the absence of amino-acids has been reported not only for specific enzymes such as β-galactosidase^{11,15}, tryptophan synthetase^{12,14,16}, enzymes of histidine operon¹⁷, and acetylornithinase and ornithine transcarbamylase¹⁸ but also for total proteins¹⁹. However, the amounts of mRNA produced depend on amino-acids used for the starvation^{12,14,16-18}.

Table 1 Effect of Amino-acid Starvation on Synthesis of Peak 11 Protein

Amino-acids removed	Synthesis of peak 11 protein
None	100
Histidine	100
Leucine	7
Threonine	30
Arginine	30

E. coli CP78(his⁻leu⁻thr⁻arg⁻) was starved for one of the required amino-acids as described in Fig. 2. After 5 min starvation, 25 µCi of ³H-arginine was added to the cultures (15 ml.) starved for histidine or leucine, and 25 µCi of ³H-leucine was added to those starved for threonine or arginine. They were incubated for another 5 min and chilled rapidly. Control experiments were carried out for both ³H-arginine and ³H-leucine as described above except for the presence of all required amino-acids. Envelope fractions were prepared and analysed by SDS gel electrophoresis, as described previously⁷. Radioactivities at the peak 11 position determined by DANS-internal standards⁸ (Fig. 2) were measured, and expressed as per cent of those of the corresponding control experiment.

Since the protein discussed here is one of the major structural proteins of *E. coli* envelope, mRNA for the protein is possibly produced constitutively. The data reported show that this constitutive production of mRNA can continue even in the absence of histidine. It will be of great interest to examine whether such mRNA synthesis has obligatory coupling with translation of the mRNA.

We have also found that the mRNA for the lipoprotein is much more stable than mRNAs for other *E. coli* proteins. Recently we examined the stabilities of mRNAs for other envelope proteins of *E. coli*, and found that of all the major structural proteins of the *E. coli* envelope the high stability of mRNA is unique only for the lipoprotein (Hirashima and Inouye, in preparation).

Similar protein synthesis during amino-acid starvation to that we have reported here has been published for coat protein

Table 2 Effects of Inhibitors of Protein Synthesis on the Production of the Lipoprotein

Inhibitors	Production of the lipoprotein
None	100%
Tetracycline (100 µg/ml.)	2%
Chloramphenicol (100 µg/ml.)	15%
Rifampicin (200 µg/ml.)	92%

The synthesis of the lipoprotein in the absence of inhibitors was carried out as described in Fig. 2. In all experiments with inhibitors, they were added when histidine starvation was started. After 5 min, ³H-arginine was added, and the mixture was incubated for another 5 min as was the control experiment. Envelope fractions were prepared, and analysed by SDS gel electrophoresis, as described previously⁷. Radioactivities at the peak 11 position determined by DANS-internal standards⁸ (Fig. 2) were measured and expressed as per cent of that of the experiment without inhibitors. *E. coli* CP78 was used for all experiments.

of RNA phage f2 (histidine starvation)²⁰, and for tryptophan synthetase A protein (tryptophan starvation)^{12,21}. In our case, only one structural protein of *E. coli* envelope is biosynthesized. This makes it possible to label specifically this lipoprotein with several unique amino-acids or amino-acid analogues without incorporating them into other envelope proteins, which is useful for structural and functional study of the protein. Furthermore, since after 4 h of histidine starvation the amount of the protein increased about three times without increasing cell mass (Fig. 3), and since the protein is metabolically very stable^{5,7}, it will be interesting to examine whether histidine starvation causes some distortion of the envelope ultrastructure due to the abnormal increase of the structural protein.

We thank Dr V. Cirillo and Mr G. Childs for reading the manuscript. This research was supported by grants from the US National Institutes of Health, the American Cancer Society, and the Research Foundation of the State University of New York, and by a fellowship from the US National Institutes of Health to A. H.

AKIKAZU HIRASHIMA
MASAYORI INOUE

Department of Biochemistry,
State University of New York at Stony Brook,
Stony Brook,
New York 11790

Received November 17, 1972.

- ¹ Braun, V., and Rehn, K., *Europ. J. Biochem.*, **10**, 426 (1969).
- ² Braun, V., and Sieglin, U., *Europ. J. Biochem.*, **13**, 336 (1970).
- ³ Braun, V., and Wolff, H., *Europ. J. Biochem.*, **14**, 387 (1970).
- ⁴ Braun, V., and Bosch, V., *Proc. US Nat. Acad. Sci.*, **69**, 970 (1972).
- ⁵ Inouye, M., Shaw, J., and Shen, C., *J. Biol. Chem.*, **247**, 8154 (1972).
- ⁶ Fiil, N., and Friesen, J. D., *J. Bact.*, **95**, 729 (1968).
- ⁷ Inouye, M., and Pardee, A. B., *J. Biol. Chem.*, **245**, 5813 (1970).
- ⁸ Inouye, M., *J. Biol. Chem.*, **246**, 4834 (1971).
- ⁹ Goldberg, A. L., *Proc. US Nat. Acad. Sci.*, **69**, 2640 (1972).
- ¹⁰ Pestka, S., *Ann. Rev. Microbiol.*, **25**, 487 (1971).
- ¹¹ Morris, D. W., and Kjeldgaard, N. O., *J. Mol. Biol.*, **31**, 145 (1968).
- ¹² Lavallé, R., and DeHauwer, G., *J. Mol. Biol.*, **37**, 269 (1968).
- ¹³ Blundel, M., Craig, E., and Kennell, D., *Nature New Biology*, **238**, 46 (1972).
- ¹⁴ Edlin, G., Stent, G. S., Baker, R. F., and Yanofsky, C., *J. Mol. Biol.*, **37**, 257 (1968).
- ¹⁵ Hall, B., and Gallant, J., *Nature New Biology*, **237**, 131 (1972).
- ¹⁶ Stubbs, J. D., and Stubbs, E. A., *J. Mol. Biol.*, **51**, 717 (1970).
- ¹⁷ Venetianer, P., *J. Mol. Biol.*, **45**, 375 (1969).
- ¹⁸ McLellan, W. L., and Vogel, H. J., *Proc. US Nat. Acad. Sci.*, **67**, 1703 (1970).
- ¹⁹ Lazzarini, R. A., and Winslow, R. M., *Cold Spring Harbor Symp. Quart. Biol.*, **35**, 383 (1970).
- ²⁰ Cooper, S., and Zinder, N., *Virology*, **20**, 605 (1963).
- ²¹ Somerville, R. L., and Yanofsky, C., *J. Mol. Biol.*, **8**, 616 (1964).

L-DOPA: Effect on Ethanol Narcosis and Brain Biogenic Amines in Mice

L-DOPA augments ethanol-induced narcosis in mice¹. Determination of brain biogenic amines suggested that the enhancement effect may be due in part to the marked increase in brain dopamine following L-DOPA administration. Here we provide further evidence to implicate dopamine in L-DOPA-induced augmentation of ethanol narcosis.

Sleeping time was defined as the length of time required for a Swiss-Webster mouse (18–23 g) to regain the righting reflex². Mice were given intraperitoneal injections of ethanol (4.0 to 7.0 g kg⁻¹). Ethanol solutions were prepared at a concentration of 25% v/v, except for the 7.0 g kg⁻¹ dose which was

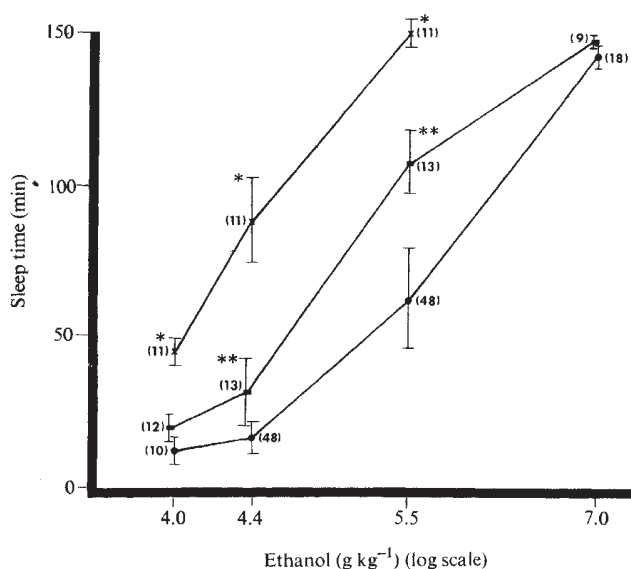


Fig. 1 Effects of acute and chronic administration of L-DOPA on ethanol-induced sleep time in mice. Standard errors of means are indicated by vertical brackets. Each point represents average data and the number of animals used in each experiment is indicated in parentheses. (*) Values significantly different from saline treatment (at least $P < 0.001$ by Student *t* test), (**) values significantly different from L-DOPA $\times$ 4 days (at least $P < 0.005$). ●, Saline+ethanol; ×, L-DOPA 400 mg kg⁻¹ $\times$ 1 day+ethanol; ■, L-DOPA 400 mg kg⁻¹ $\times$ 4 days+ethanol.

given as a 50% v/v solution so that no mouse received a volume greater than 0.75 ml. Duration of ethanol-induced sleep time was compared for groups of mice pretreated with intraperitoneal injections of saline or L-DOPA. L-DOPA methyl-ester was dissolved in saline and administered as the free base.

Mice were given one of the following drug treatments: (1) a single administration of saline, (2) saline daily for four days, (3) a single administration of 400 mg kg⁻¹ of L-DOPA, or (4) L-DOPA (400 mg kg⁻¹) daily for four days. All animals received an injection of ethanol 45 min after the last drug treatment.

Brain norepinephrine (NE) was determined by the procedure of Anton and Sayre³ and DOPA and dopamine (DA) by the method of Merritt and Schultz⁴. Analysis of ethanol in the blood and brain was carried out in other groups of mice subjected to the same pretreatment conditions, but only 4.4 g kg⁻¹ of ethanol was used. Blood and brain samples (4 mice) were taken from the three groups at their respective average duration of sleep and were analysed for ethanol⁵.

Sleep times between groups 1 and 2 did not differ significantly⁶, so data derived from these groups were pooled (Fig. 1). Treatments 3 and 4, before ethanol, resulted in sleep time curves which were shifted significantly (at least $P < 0.05$) to the left of the saline-ethanol sleep time curve. A single injection of L-DOPA produced the maximum augmentation of ethanol-induced sleep time. L-DOPA for four days resulted in a lesser augmentation (at least $P < 0.05$).

L-DOPA for one day showed a marked increase in both brain DOPA (192-fold, $P < 0.001$) and DA (11.1-fold, $P < 0.001$) and a lesser increase in brain NE (0.76-fold, $P < 0.01$) (Fig. 2). Similar changes have been reported by other investigators^{7,8}. L-DOPA for four days resulted in an increase in brain DOPA (87-fold, $P < 0.001$), DA (3.4-fold, $P < 0.001$) and NE (1.7-fold, $P < 0.001$). Although brain levels of the respective catecholamines were increased, chronic L-DOPA significantly reduced the rise in DOPA and DA levels while increasing NE content when compared to data obtained with a single injection of L-DOPA.

Ethanol in the blood or brain after saline and ethanol did not differ significantly from that obtained after L-DOPA for 1 day and ethanol, or L-DOPA for 4 days and ethanol, despite

Table 1 Effect of Acute and Chronic L-DOPA Treatment on Blood and Brain Alcohol Levels

Treatment	Blood alcohol mg% $\pm$ s.e.	Brain alcohol mg% $\pm$ s.e.
L-DOPA 400 mg kg ⁻¹ 1 day	170.3 $\pm$ 10.9	207.3 $\pm$ 17.0
L-DOPA 400 mg kg ⁻¹ 4 days	216.0 $\pm$ 26.8	238.3 $\pm$ 10.4

the fact that with the single injection of L-DOPA the animals slept longer than the chronic L-DOPA group (Table 1).

L-DOPA enhanced the soporific action of ethanol in mice. This effect was reduced when L-DOPA was given for four days rather than one day. We suggest the increase in brain DA following L-DOPA indicates that the enhancement effect may be due in part to a dopaminergic mechanism, and a direct correlation exists between the intensity of the augmentation effect and the rise in brain DA level following L-DOPA. As the rise in brain DA level is reduced following chronic L-DOPA, the intensity of enhancement of ethanol sleep time is reduced.

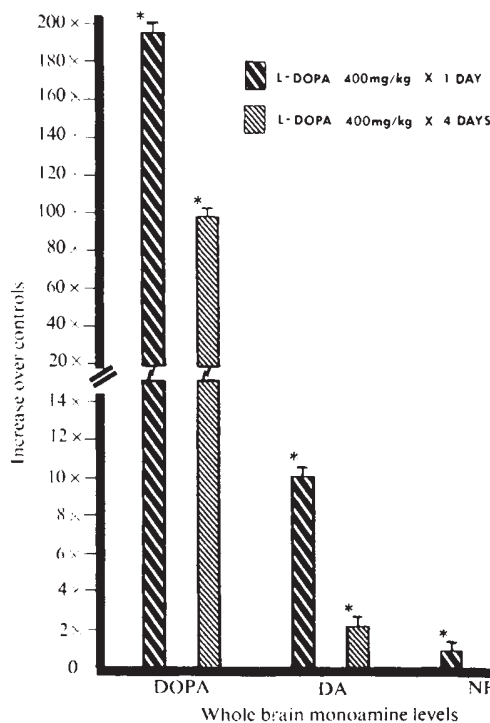


Fig. 2 Effects of acute and chronic administration of L-DOPA on mouse whole brain monoamine content. Standard errors of means are indicated by vertical brackets. Each value is an average of at least 5 determinations read in triplicate. (*) Values significantly different from saline treatment (at least $P < 0.05$ by Student t test).

The role of increased NE (0.76-fold) after the single injection of L-DOPA in the effects of ethanol appears to be quantitatively less important than increased DA (11.1-fold). Although NE increased 1.7-fold following chronic L-DOPA, enhancement of ethanol-induced narcosis was significantly reduced. Using L-DOPA to replenish α -MPT-reduced stores of the catecholamines⁹, it replenished NE stores, increased DA ($P < 0.05$, 60%) and partially protected the α -MPT-induced enhancement of sleep time. We suggest that DA, rather than NE, is more involved in ethanol-induced sleep.

Three groups of mice (10 each) received saline, DA (10 mg kg⁻¹) or disulfiram, a beta hydroxylase inhibitor (400 mg kg⁻¹). Ethanol (4.0 g kg⁻¹) was injected with saline or dopamine, or four hours after disulfiram or saline. DA, observed

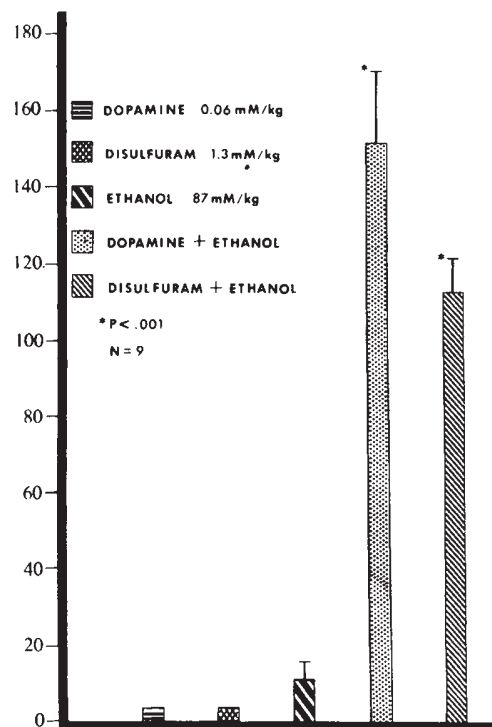


Fig. 3 Effects of dopamine and disulfiram on ethanol-induced sleep time in mice. Nine mice divided into three groups received saline, DA (10 mg kg⁻¹) or disulfiram (400 mg kg⁻¹). Ethanol (4.0 g kg⁻¹) was injected with saline or dopamine, or four hours after disulfiram or saline. Standard errors of means are indicated by vertical brackets. (*) Indicates values significantly different from saline treatment ($P < 0.001$).

previously¹⁰, and disulfiram treatment potentiated ($P < 0.001$) sleep time induced by ethanol (Fig. 3). Disulfiram reportedly decreased brain NE by 56% and increased DA by 14%¹¹; in our experiment, it produced a temporary increase in the ratio of DA/NE resulting in DA > NE and the accompanying enhanced ethanol-induced behavioural depression. The synergistic action of disulfiram, however, may be due to its effect on aldehyde dehydrogenase¹². Our results indicate that L-DOPA induced enhancement of ethanol sleep is due to the rise in brain DA.

L-DOPA is converted by a pyridoxal phosphate-dependent decarboxylation reaction to DA¹⁴ and reacts with pyridoxal phosphate *in vitro*¹⁵. Removal of pyridoxal phosphate by chronic L-DOPA possibly lessened decarboxylase activity with a decreased conversion of DOPA to DA. Diminished formation of DA, however, may be due to decreased brain L-DOPA uptake.

We thank B. Wiggins, J. Georgacakis, J. Landez, and M. Mendoza for technical help. The work was supported by NIH and Air Force Office of Scientific Research grants.

K. BLUM
W. CALHOUN

Department of Pharmacology,
University of Texas Health Science Center at San Antonio,
San Antonio,
Texas

J. MERRITT
J. E. WALLACE

Forensic Toxicology Branch,
Brooks Air Force Base,
San Antonio, Texas

Received September 18; revised October 31, 1972.

- ¹ Blum, K., Merritt, J. H., Sulkowski, T., and Wallace, J. E., *Fifth Internat. Cong. Pharmacol.*, 1972 (abstract).
- ² Kakihana, R., Brown, D. R., McClearn, G. E., and Tabershaw, I. R., *Science*, **154**, 1574 (1966).
- ³ Anton, A. H., and Sayre, D. R., *J. Pharmacol. Exptl. Therap.*, **138**, 360 (1962).
- ⁴ Merritt, J. H., and Schultz, E. J., *Life Sci.*, **5**, 27 (1966).
- ⁵ Wallace, J. E., and Dahl, E. V., *Amer. J. Clin. Pathol.*, **46**, 152 (1966).
- ⁶ *Standard Mathematical Tables*, 250 (Chemical Rubber Publishing Co., Cleveland, Ohio, 1963).
- ⁷ Everett, G. M., and Borcharding, J. W., *Science*, **168**, 849 (1970).
- ⁸ Hanig, J. P., and Seifter, J., *Experientia*, **27**, 168 (1971).
- ⁹ Blum, K., Merritt, J. H., Wallace, J. E., Owen, R., Hahn, J. W., and Geller, I., *Current Therapeutic Research*, **14**, 324 (1972).
- ¹⁰ Rosenfeld, G., *Quart. J. Stud. Alc.*, **21**, 584 (1960).
- ¹¹ Goldstein, M., and Wakajima, K., *J. Pharm. Exptl. Therap.*, **157**, 96 (1967).
- ¹² Jacobson, E., *Brit. J. Addiction*, **47**, 26 (1950).
- ¹³ Spector, S., Gordon, R., Sjoerdsma, A., and Udenfriend, S., *Mol. Pharmacol.*, **3**, 549 (1967).
- ¹⁴ Duvoisin, R. C., Yahr, N. D., and Cote, L. D., *Trans. Amer. Neurol. Ass.*, **94**, 81 (1969).
- ¹⁵ Tjandramaga, T. B., Goldbert, L. I., and Anton, A. H., *Fifth Internat. Cong. Pharmacol.*, 1972 (abstract).

Binding of Concanavalin A to the Surface of Unfertilized and Fertilized Ascidian Eggs

STUDIES with certain carbohydrate-binding proteins of plant origin (as concanavalin A and wheat germ agglutinin) have indicated that some surface properties of embryonic^{1,2} and of virally transformed cells³⁻⁶ are different from those of adult cells. The interpretation of the differences in terms of structural organization of the cell surface is, however, still controversial (see for example refs. 7 and 8).

We have used concanavalin A (Con A) as a tool to explore the changes taking place in the surface of the Ascidian egg in conjunction with fertilization. Indeed, while it is well documented by studies both at the morphological and the physiological level (see ref. 9 for a review) that fertilization brings about an extensive reorganization of the egg surface, whether and to what extent these changes are relevant to the onset of development or only to the process of sperm-egg interaction is not known.

In this paper we show that in the Ascidian egg at the time of completion of maturation (which coincides with the re-acquisition by the egg of the ability to divide) a dramatic change occurs in the detectability of Con A binding sites on the egg surface.

The choice of the Ascidian egg to explore the changes of the egg surface at the time of fertilization by the use of molecular probes was dictated by the following considerations. The Ascidian egg is among the few eggs well suited for microscopical analysis (the eggs of the Mediterranean species are very transparent and have a diameter of about 100–140 μm) whose chorion can be manually removed, thus exposing a naked egg surface; neither by conventional electron microscopy nor by staining with ruthenium red can an extracellular coat be detected. This is an essential prerequisite for use of molecular probes to provide meaningful information about the cell surface.

In the Ascidian oocyte, the meiotic process is arrested at the metaphase of the first division and is only resumed following fertilization. Within 20–30 min after fertilization, meiosis is completed and the two polar bodies are ejected. The male and female pronuclei then merge in the zygote nucleus and soon afterwards the egg undergoes the first cleavage. Immediately following fertilization, the egg surface appears to be stretched at the animal pole and to contract at the vegetal pole (which is the site of sperm entry)¹⁰. At the same time the egg becomes sensitive to taurocholate-induced cytolysis¹¹.

Rapid cytoplasmic movements also follow fertilization; they cause the egg to undergo amoeboid-like deformations¹². After the ejection of the second polar body, these movements lead to the segregation of cytoplasmic components into specific areas and eventually into the blastomeres from which the different cell lines originate¹³.

We have used eggs of *Ascidia malaca* and of *Phallusia mamillata* collected from the gonoducts; the chorion was removed with tungsten needles under a dissecting microscope.

Fluorescein isothiocyanate-conjugated Con A (a gift of Dr L. Sachs, the Weizmann Institute) was diluted in seawater to a final concentration of 70 $\mu\text{g ml}^{-1}$. Eggs and cleavage stages stained with fluorescent Con A often underwent cytolysis during observation if a seawater-glycerol mountant was used. This could be prevented by a 10-min fixation in isotonic 3% neutral formaldehyde in seawater. The eggs, both fixed and unfixed, were stained for 10 min and rinsed in seawater before observation. Comparison with unfixed material showed that fixation did not alter Con A binding. For the observation of fluorescence a Zeiss Universal microscope was used with an Epicondenser and standard ultraviolet filters for fluorescein. The eggs of *A. malaca* are slightly autofluorescent, but negligibly so when compared with the stained specimens. *Phallusia* eggs, however, showed considerable autofluorescence near the wavelength of fluorescein emission; for this reason most of our observations were done with *A. malaca* eggs. Agglutination tests were performed with Con A (Calbiochem) in concentrations of 50 to 500 $\mu\text{g ml}^{-1}$ in seawater.

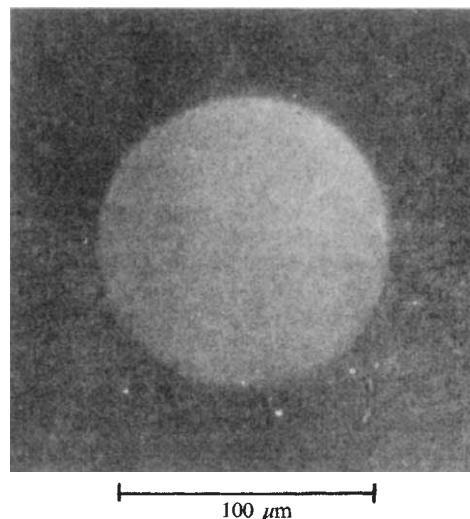


Fig. 1 Unfertilized egg of *Ascidia malaca* treated with trypsin and then exposed to fluorescein-conjugated Con A.

Unfertilized eggs of *A. malaca* are not agglutinated by Con A at these concentrations and no binding of fluorescent Con A is observed. A slight degree of agglutination is exhibited by the unfertilized eggs of *Phallusia*; however, the clumps are readily dispersed on gentle shaking of the dishes. Until the ejection of the second polar body, no Con A binding can be detected either by the agglutination or by the fluorescent test. However, a 30-min treatment with 0.1% Trypsin (Worthington) results in agglutination when the eggs are subsequently exposed to a 50 $\mu\text{g/ml}$ solution of Con A; also, binding of fluorescent Con A becomes detectable (Fig. 1). In the case of the *Phallusia* eggs, the clumps of trypsin-treated agglutinated eggs do not disperse on shaking of the dishes.

Immediately following the ejection of the second polar body, Con A strongly agglutinates the eggs in clumps that are not dispersed by shaking the dishes. The sudden appearance of agglutinability is most strikingly demonstrated when eggs are exposed to a 50 $\mu\text{g ml}^{-1}$ solution of Con A at any time

between fertilization and the formation of the second polar body; indeed the maturation process continues in the presence of Con A and the eggs do not show any tendency to agglutinate. As soon as the second polar body has been given off, agglutination immediately occurs. Concurrently, treatment with fluorescent Con A results in the appearance of a bright fluorescence (Fig. 2), which is in fact brighter than that of the unfertilized eggs treated with trypsin. Both agglutination and fluorescence are abolished by α -D-methyl-manno-pyranoside, showing that they are due to the specific binding of Con A to its receptor sites¹⁴.

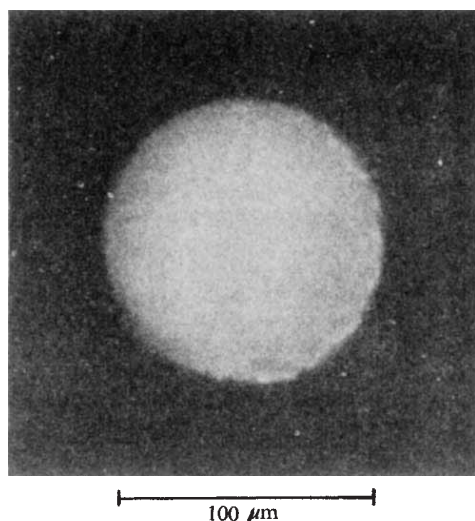


Fig. 2 Egg of *Ascidia malaca* exposed to fluorescent Con A immediately after the ejection of the second polar body.

We interpret these observations as indicating that, in the Ascidian egg, completion of maturation is accompanied by a molecular rearrangement of the egg surface. Quantitative estimates of the Con A binding sites before and after the ejection of the second polar body are in progress. Hence for the time being we cannot say whether our results are due (1) to an all-or-none exposure of the Con A binding sites, or (2) to an increase of the available sites, or (3) to their topological rearrangement (see refs. 5, 6 and 15). The following points are pertinent to the interpretation of our observations.

Table 1 Changes in Diameter of the Eggs of Four *Ascidia malaca* at the Time of Fertilization

Experiment No.	No. of eggs	Unfertilized eggs Diameter (μm)	σ	After ejection of second polar body Diameter (μm)	σ
1	8	112.4	0.02	109.7	2.09
2	10	106.2	0.52	103.7	1.31
3	21	107.9	<0.001	107.3	0.13
4	4	109.2	0.64	107.9	<0.001

There is a small variability in size not only among the eggs of different animals but also among those from the same animal, so the data in the table have been computed from the measurements of the diameter of individual eggs before fertilization and after the ejection of the second polar body.

(1) Since it is known that the eggs of some animals undergo a marked shrinkage on fertilization^{9,11}, the possibility should be considered that the appearance of fluorescence and of agglutinability may be due to a clumping of previously scattered Con A binding sites. This, however, is made unlikely by the data in Table 1 which show that while the eggs of some animals do not undergo any change in size after fertilization, in other

cases only a slight decrease occurs. Furthermore, it seems that, when all the eggs are considered together, in 65% of them no change in diameter after fertilization can be detected; a decrease of between 3.6 and 4.1% occurs in 30% of the eggs; and only in 5% of the eggs is the decrease of about 7%. We conclude that such size changes, even when they take place, are too small to account for a shrinkage-dependent clustering of the Con A binding sites.

(2) The fact that Con A binding sites become detectable by the use of fluorescent Con A at the surface of the unfertilized egg as a result of trypsin treatment suggests that at least one part of the sites is in a cryptic condition. It is interesting that neither of the changes that occur at the egg surface immediately after fertilization, namely the sensitivity to taurocholate¹¹ and the surface movements¹⁰, involves exposure of the Con A binding sites. This means that it is only after maturation has been completed that the molecular architecture of the egg surface is altered in such a way as to permit detection of the Con A binding sites (at least by the methods we have employed). Whether or not this cell surface change is causally related to the resumption by the egg of the ability to replicate its DNA and to divide cannot be decided on the basis of the present experiments. Preliminary observations indicate that no further changes in the cell surface occur with respect to Con A binding in the course of cleavage.

The initial experiments described in this paper were carried out at the Zoological Station, Naples. We thank Drs J. Brachet, G. M. Edelman, G. Geraci and G. Marin for discussions and suggestions.

A. MONROY
G. ORTOLANI*
D. O'DELL†
G. MILLONIG

CNR Laboratory of Molecular Embryology,
Arco Felice,
Naples

Received December 19, 1972.

* Present address: Istituto di Zoologia, University of Palermo, Italy.

† Present address: Department of Zoology, University College, London.

- Moscona, A. A., *Science*, **171**, 905 (1971).
- Kleinschuster, S. J., and Moscona, A. A., *Exp. Cell Res.*, **70**, 397 (1972).
- Burger, M. M., *Proc. US Nat. Acad. Sci.*, **62**, 994 (1969).
- Inbar, M., and Sachs, L., *Proc. US Nat. Acad. Sci.*, **63**, 1418 (1969).
- Inbar, M., Ben-Bassat, H., and Sachs, L., *Nature New Biology*, **236**, 3 (1972).
- Burger, M., *Curr. Topics in Cell Reg.*, **3**, 135 (1971).
- Cline, M. J., and Livingston, D. C., *Nature New Biology*, **232**, 155 (1971).
- Ozanne, B., and Sambrook, J., *Nature New Biology*, **232**, 156 (1971).
- Monroy, A., *Chemistry and Physiology of Fertilization* (Holt, Reinhart and Winston, New York, 1965).
- Ortolani, G., *Riv. Biol.*, **47**, 169 (1955).
- Monroy, A., *Pubbl. Staz. Zool. Napoli*, **25**, 188 (1954).
- Reverberi, G., *Pubbl. Staz. Zool. Napoli*, **15**, 198 (1936).
- Conklin, E. G., *J. Acad. Nat. Sci. Philadelphia*, **13**, 1 (1905).
- Sharon, N., and Lis, H., *Science*, **177**, 949 (1972).
- Nicolson, G. L., *Nature New Biology*, **239**, 193 (1972).

Hormonal Pregnancy Tests and Spina Bifida

GAL¹ has suggested that hormonal pregnancy tests may be teratogenic to the developing central nervous system. In a study of 100 mothers of spina bifida cases she found nineteen of these

mothers had had a hormonal pregnancy test ('Amenorone Forte' or 'Primodos') compared with four of 100 "matched" control mothers. Laurence² has criticized Gal's findings on two counts: (1) the choice of controls, and (2) the possibility of finding a difference this great by chance when a number of epidemiologic factors are studied.

An examination of the data presented by Gal¹ suggests an important factor in the pathogenesis of spina bifida may have been overlooked. Gal states that the average interval between conception and pregnancy test was 5.6 weeks for the survey group and 6.2 weeks in the control group. If the index cases were open lesions of the spine (myeloceles or myelomeningoceles), which are the most common forms of spina bifida, these would develop during the first phase of neural tube formation³. The neural tube is formed and closed by 28 days gestational age³ so any teratogenic factor would need to be operating before this time. The average interval between conception and pregnancy test in the study group was 5.6 weeks, implying that a large proportion of these mothers received the hormones after a teratogen could affect the closure of the neural tube, that is, after the 4th week. For Gal's findings to be biologically significant, the time of operation of the proposed teratogen must be more precisely established and be before the closure of the neural tube.

LOWELL E. SEVER

*Department of Epidemiology,
University of Washington,
Seattle, Washington 98195*

Received December 11, 1972.

¹ Gal, I., *Nature*, **240**, 241 (1972).

² Laurence, K. M., *Nature*, **240**, 242 (1972).

³ Lemire, R. J., *Teratology*, **2**, 361 (1969).

Spatial Frequency Dependent Chromatic After-effects

McCOLLOUGH¹ first reported that following prolonged inspection of vertical gratings on orange backgrounds successively alternating with horizontal gratings on blue backgrounds, the vertical stripes in an achromatic test pattern will appear greenish and the horizontal stripes will appear pink. Because the effect was evidently related to the orientation of the grids on the retina, McCollough hypothesized that it was due to adaptation or fatiguing of colour- and orientation-specific edge detectors in the visual system. But recent studies²⁻⁴ employing two grids of varying spatial frequency but with the same orientation have demonstrated similar chromatic after-effects on achromatic test patterns of the same orientation.

My studies examined the interdependence of spatial frequency and orientation-specificity of chromatic after-effects. It was hypothesized that chromatic after-effects could be produced with stimulus attributes totally independent of orientation.

Six subjects who were familiar with the McCollough effect but were naive with respect to the hypothesis of my experiment viewed two black horizontal gratings with a spatial frequency of five cycles/degree and 10 cycles/degree respectively. The narrow gratings were projected through green filters (Wratten 52), the wide gratings through magenta filters (Wratten 34A). The 35 mm slides were viewed binocularly through a projection tachistoscope; the image was projected on ground glass by two Kodak carousel projectors. The 40 training trials consisted of alternate presentations of the slides for 7 s with a dark interval of 5 s. Five minutes after this training period, subjects viewed a variable spatial-frequency grid, ranging from 2 to 20 cycles/degree projected on a white screen. Half saw this test grid in a horizontal orientation first; the other half saw it in a vertical

orientation first. The grid was then rotated 90°. Subjects were asked to report any colour effects observed in either or both orientations. If an effect was observed, subjects were instructed to locate the point of maximum chromatic saturation on the grid. They were also asked to compare the strengths of the effects at this point of maximum saturation in the two orientations.

All subjects reported a greenish tint on the narrow-spatial frequency portions and a reddish tint on the wide-spatial frequency portions for both orientations of the test grid, with an achromatic band between the two sections. The effect was strongest near the spatial frequency which corresponded to that of the training slide. Subjects reported a decreased (desaturated) effect as the spatial frequency deviated in either direction from that of the training grids. Qualitatively, they judged the strength of the effect to be similar in both orientations.

In a second experiment, the training procedure was identical, except that the training stimuli were concentric circles, one with a spatial frequency of 5 cycles/degree and the other 7.5 cycles/degree. The test stimulus consisted of a series of 22 concentric circles varying in spatial frequency from one cycle/degree at the centre to 15 cycles/degree at the periphery. Five subjects from the previous experiment and three new subjects participated in this experiment. Half viewed the narrow circles through red and the wide circles through green filters, and the order was reversed for the other subjects. A five minute interval separated the training and test phases. Subjects were instructed to report colour effects on the test pattern and to indicate the region of maximal saturation.

All subjects reported colour bands extending around the entire circumference of the circles. Those who had viewed narrow green circles and wide red circles reported a reddish or pink tint on the narrowly spaced portion and a greenish tint on the widely spaced portion of the test pattern. Analogous after-effects were reported by those subjects who had viewed the alternately paired training stimuli. Three experienced subjects commented that the effect was considerably weaker than those observed in the previous experiment.

The results of both experiments argue in favour of independently functioning channels for the processing of orientation and spatial frequency attributes of visual stimuli. These findings suggest that explanations based on the response characteristics of single units become increasingly less parsimonious. Orientation specificity is a defining characteristic of edge detectors and to a lesser extent seems to apply to size detectors as well^{5,6}. Parker⁷ argued for the existence of a class of neurones with orientation specific tuning but responding to a range of spatial frequencies. The present findings would require yet a further class of units which are spatially tuned but nonspecific with respect to orientation. To account for the chromatic after-effects, units in each of these classes would also have to be tuned to specific wavelength band, become "fatigued" as a result of prolonged stimulation as in the McCollough effect, and produce complementary responses.

It seems that no explanation of the effect based on a single neural unit can be adequate. Both line orientation and spatial frequency seem to be sufficient but not necessary stimulus attributes to produce an after-effect. A more adequate, yet physiologically nonspecific, model can be developed from these and other findings. I propose that an association occurs between some attribute of the stimulus structure, for example, orientation, and the chromatic characteristics of the training stimuli. Because the after-effect appears as a hue approximately complementary to the hue of the training stimuli, an association between the structural aspects and an opponent colour response must occur. A learning model analogous to classical conditioning appears to have considerable heuristic, if not explanatory, value. Physiologically it may be speculated that chromatic information is relayed to cortical centres via neural units in the lateral geniculate nuclei which respond in a manner consistent with an opponent process⁸ to form an

association with the contour related information processed by appropriate cortical neural units.

Work supported by a grant from the National Research Council, Canada.

P. K. LEPPMANN

University of Guelph, Ontario

Received July 10; revised December 7, 1972.

¹ McCollough, C., *Science*, **149**, 1115 (1965).

² Harris, C. S., *J. Opt. Soc. Amer.*, **61**, 689 (1971).

³ Lovegrove, W. J., and Over, R., *Science*, **176**, 541 (1972).

⁴ Murch, G. M., and Hirsch, J., *Amer. J. Psychol.*, **85**, 241 (1972).

⁵ Hubel, D. H., and Wiesel, T. N., *J. Physiol.*, **195**, 215 (1968).

⁶ Campbell, F. W., and Kulikowski, J. J., *J. Physiol.*, **187**, 437 (1966).

⁷ Parker, D. M., *Quart. J. Exp. Psychol.*, **24**, 1 (1972).

⁸ De Valois, R., *J. Gen. Psychol.*, **43**, 115 (1960).

The Role of Contours in Stereopsis

JULESZ¹ showed that the presence of monocular patterns or familiar shapes is not necessary for stereopsis and that correlated noise matrices presented binocularly can yield vivid depth effects. Random dot stereograms "demonstrated that stereopsis not only can occur without monocularly recognizable shapes but actually does occur. That is, if monocular shapes exist then stereopsis precedes their recognition . . .". Julesz also suggested that stereopsis may result

from simple point to point matching² of the left and right eye fields. Experiments with random letter stereograms⁴ and gratings⁵ emphasized the importance of brightness gradients in stereopsis and Kaufman concluded that "subjective contours" are not sufficient to produce stereopsis.

The problem is: what features of a pair of patterns can define the disparity that gives rise to the perception of stereopsis? We investigated whether disparity of visual texture is sufficient in itself to produce stereopsis using Fig. 1.

Pickett⁶ found that (in random dot matrices) texture discrimination can occur if the density of elements (first order of probability) is kept constant and the tendency for like elements to occur in "runs" (second order of probability) is varied, in spite of there being no differences in average intensity. We constructed stereogram 1 (Fig. 1) using a "texture discrimination" pattern from a publication of Pickett⁶. Discrimination of nasally displaced inner square in each half image is based on a difference in second order statistics alone.

We found that the brain can discriminate textures such as those shown in Fig. 2 based on granularity or size of the dots. In stereogram 2 (Fig. 2) the patterns are random "noise"; the outer area in each half image is simply the enlarged version of the inner nasally displaced texture. As both white and black elements are equally enlarged to produce the outer areas, the average intensities of outer and inner areas are equal. In spite of this, discrimination is possible.

In both these stereograms the two half images are com-

Fig. 1 Texture disparity stereogram. Texture discrimination based on differences in second order statistics.

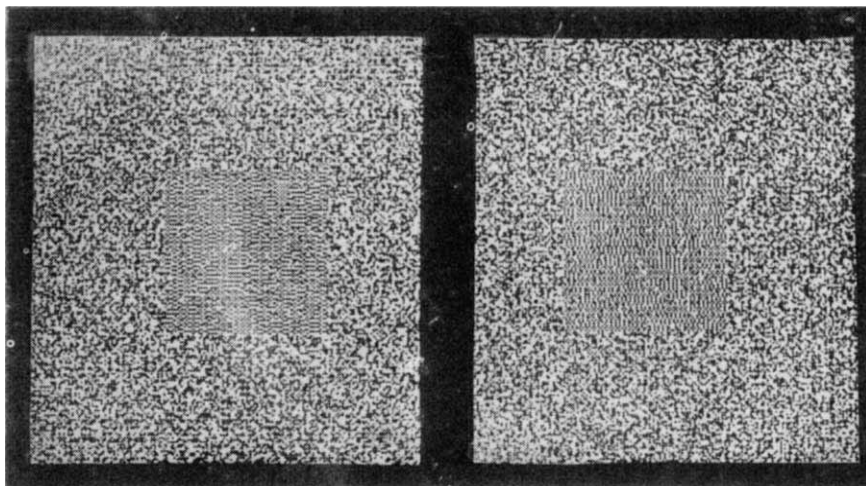


Fig. 2 Texture disparity stereogram. Texture discrimination based on size of grain. This is an inaccurate version of the original stimuli used in our experiment. In the original stereogram all intensity contours were eliminated using white paint and appropriate illumination.

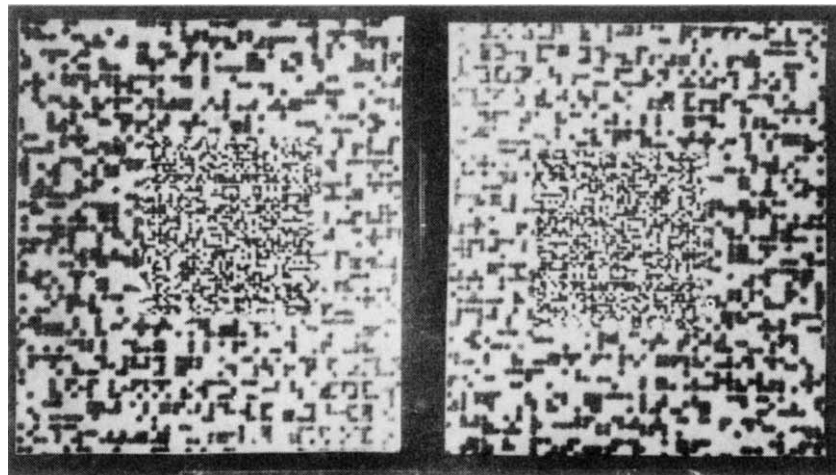
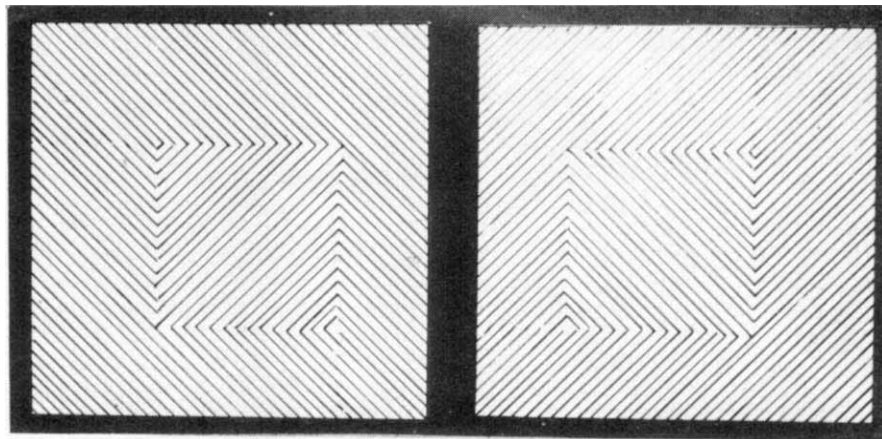


Fig. 3 Disparity of subjective contours formed by grating direction change



pletely random uncorrelated noise. The spatial distribution of dots in one half image has nothing to do with the spatial distribution of dots in the other half image. In each half image, however, there is a discriminable central patch shifted nasally. Are the discrimination and disparity of such abstract "gestalt contours" as these sufficient for stereopsis, although with respect to dot distribution, pattern *a* has no point to point correlation with pattern *b*?

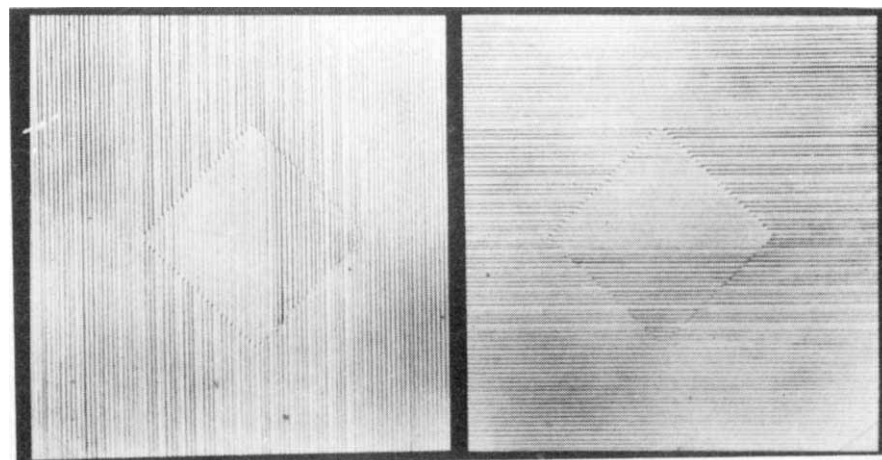
Stereograms (Figs. 1 and 2) with nasal and temporal disparities were presented in random order to 6 trained O's (20 trials for each O) and they were provided with zero disparity "controls" for comparison. All O's reported stereopsis with Fig. 1 in the correct direction for 100% of trials. The accuracy was 95% for Fig. 2 (but the "quality" of stereopsis was poor).

front of or behind that of the outer square. This result is of particular interest, for it suggests that there might be a common contour processing mechanism in the brain for both subjective "texture" contours and intensity contours.

In Fig. 3 there are subjective contours formed by grating direction change. All 6 O's reported stereopsis in the correct direction for 100% of trials when presented with crossed and uncrossed disparities in random order for 20 trials each. Kaufman's negative result⁵ with similar stereograms could probably be explained by the fact that the disparities he used were too small (for the grating band-width which he used).

Treisman⁷ and Julesz³ have shown that colour can provide a cue for stereopsis. This seems to be true even in the absence of point to point correlation between two half

Fig. 4 Disparity of subjective contours formed by Vernier resolution.



This result is all the more surprising in view of the fact that in "noise" patterns presented binocularly each dot in the left eye usually selects the nearest available partner dot from the corresponding area of the other eye for fusion and yields stereopsis depending on the disparity of the selected partner dot. In the absence of other cues proximity seems to be the cue for stimulus selection in stereopsis. Why does the proximity rule fail when subjective contours are presented in noise patterns?

We also presented a "texture contour" such as the one used in Fig. 1 to the left eye and an intensity contour (formed between a light grey surround and dark grey inner square) to the right eye. Stereograms of this type with crossed and uncrossed disparity were presented in random order 20 times to each of 6 trained O's. Accuracy of reported direction of depth was 100% for three of the O's, 90% for two of the O's and 70% for one. The combined image was reported to be diplopic, rivalrous and unstable, but the border of the inner square was clearly either in

images⁸. In Fig. 4 one half image consists of a dark grey outer area and a light grey nasally displaced inner area. In the other half image the outer area is red and the inner area is green (Wratten Green Filter No. 58). The intensity of the inner green area was varied gradually over a wide range using a pair of polaroids. At some setting, at least, the intensities of the outer and inner areas must have been equal, thereby eliminating incidental intensity gradients. Yet none of the 3 O's who viewed the stereogram reported disappearance of stereopsis at any point, showing that a "pure" colour contour can produce stereopsis with an intensity contour. The same set-up was used with colour contours presented to both eyes to show that "pure" colour contours can interact binocularly to produce stereopsis (The intensities of the green areas in the 2 half images were varied synchronously.)

The validity of Julesz's model for stereopsis as far as correlated patterns are concerned cannot be doubted¹⁻³. As in our stereograms (Figs. 1 and 2), however, the half images

are completely uncorrelated "noise", Julesz's model is inapplicable. Further, Kaufman's brightness averaging hypothesis^{4,5} cannot be applied to our patterns (Figs. 1, 2 and 3) as "brightness averaging" could have resulted only in a uniform grey field in each eye.

It seems likely from our results that even the most "subjective" of subjective contours can yield depth effects in the absence of average brightness gradients and point to point correlation. Texture discrimination based on probability distributions (Fig. 1) and size of grain (Fig. 2) and the recognition of contours formed by changes in grating direction (Fig. 3) all occur "earlier" than stereopsis and considerably influence it. Any model that tries to "explain" stereopsis must take these facts into account. Our experiments also emphasize the importance of using random dot stereograms in perceptual research, for only by using Julesz patterns can one "skip" peripheral preprocessing (of the type seen in Figs. 1, 2 and 3) to obtain a truly cyclopean "counterpoint".

From our finding that a texture contour or a colour contour presented to one eye can produce stereopsis with an intensity contour presented to the other eye, we postulate a common contour processing centre for intensity, colour and texture contours where parallel inputs become confluent into a single channel. Sherrington postulated⁹ over 60 years ago "that during binocular regard . . . each monocular mechanism develops independently a sensual image of considerable completeness. The singleness of binocular perception results from union of these elaborated unocular sensations". Our results strongly support this important conclusion.

We thank Professors W. A. H. Rushton and P. Hariharan and Drs H. Asher, O. J. Braddick and C. Blakemore for their help and stimulating discussions.

V. S. RAMACHANDRAN
V. MADHUSUDHAN RAO
T. R. VIDYASAGAR

Madras Biomedical Association,
Stanley Medical College,
Madras

Received March 27, 1972; final revision January 22, 1973.

¹ Julesz, B., *Science*, **145**, 356 (1964).

² Julesz, B., *Foundations of Cyclopean Perception* (University of Chicago Press, Chicago and London, 1971).

³ Julesz, B., *Sci. Amer.*, **212**, 38 (1965).

⁴ Kaufman, L., *Amer. J. Psychol.*, **77**, 393 (1964).

⁵ Kaufman, L., *On the Operations Underlying Stereoscopic Combination*, Special Publication, Sperry Rand Research Center, Massachusetts (1966).

⁶ Pickett, R. M., *Visual Analyses of Texture in the Detection and Recognition of Objects, Picture Processing and Psychopictorics* (edit. by Lipkin, B. S., and Rosenfeld, A.) (Academic Press, New York, 1970).

⁷ Treisman, A., *Quart. J. Exp. Psychol.*, **14**, 23 (1962).

⁸ Ramachandran, V. S., Madhusudhan Rao, V., Vidyasagar, T. R., and Sriram, S., *Vision Res.* (in the press).

⁹ Sherrington, C. S., *Integrative Action of the Nervous System* (Yale University Press, New Haven, 1906).

Locomotion and Burrowing in Limbless Vertebrates

GAYMER¹ recently described "vermiform" movement as a fifth^{2,3} and "new" method of locomotion in limbless terrestrial vertebrates. On the basis of X-rays of limbless amphibians (caecilians) he stated that these animals can shorten their bodies by flexing the vertebral column into multiple curves of

short radius within the integumentary envelope. The shortened region is apparently fixed against the soil or tunnel walls and the head then moves forward from this stationary zone. The fixed region thus serves in force transmission to the soil, allowing the animal to pull up its posterior trunk, or send the anterior portion further along the path. Gaymer noted that the fixed zone thus formed may also serve as a base for ramming movements.

The shortening and thickening of the caecilian trunk by axial flexure have been known, if inadequately understood, for some decades⁴. Indeed, vermiform locomotion superficially resembles the movement of annelids, where part of the trunk is thickened and placed in static frictional contact with the walls of the tunnel. Forces transmitted here can move the body along a tunnel or let the animal push its way through the ground. These properties of vermiform motion are, however, those that characterize powered concertina movements, supposedly a distinct category^{2,3}. The difference between the usual concertina and vermiform forms of locomotion is that while the entire body flexes in the former, flexion is restricted to an axial mass in the latter so that the supervening soft tissues swell outwards. Thus vermiform locomotion is a variant of the concertina locomotion of limbless vertebrates; it is not a distinct method.

Gaymer's description of vermiform locomotion argues that such a pattern could not be developed in reptiles because their ribs extend into the body wall muscles close to the skin. However, this variant occurs in a number of limbless reptiles. Thus the slender and bilaterally compressed amphisbaenian *Agamodon compressum* uses it in tunnelling through packed sand. In the Uropeltidae, a relict family of snakes restricted to Central India and Sri Lanka (Ceylon)⁵, this method is most highly developed; it is used not only in progression and ramming but also in tunnel widening. The anterior part of the body can, by flexion of the vertebral column, be thickened to more than twice the normal diameter. In this case the basic concertina mechanism facilitates tunnel formation. The tiny head is driven in only to the level of the neck and thus serves primarily for the initial penetration. Widening of the tunnel to full diameter occurs by flexion between the anterior vertebrae, so that the penetrating and the widening functions are separated⁶.

Finally, Gaymer states that "the skin of reptiles is largely free from the underlying muscles for part or all of the body's circumference, being attached only by specially developed cutaneous muscles".

Amphisbaenians and such snakes as uropeltids do indeed have cutaneous muscles (that is, those cross-connecting different portions of the skin⁷), but these are not involved in the propulsion phase of rectilinear movement, which is enabled by slips of the axial musculature (Mm. costocutanei superiores and inferiores^{3,8}). In concertina, propulsive forces pass directly from the independently flexed vertebral column to the loosely connected skin. Behavioural evolution, coupled with only slight structural changes, accounts for repeated and varied development of concertina locomotion in reptiles.

Internal concertina is produced differently in squamates and caecilians. In uropeltids most bones, axial muscles, and viscera flex within an envelope consisting of the skin and cutaneous muscles. In caecilians the flexure is restricted to a central zone occupied by vertebrae, ribs, and spinal musculature (epaxial + hypaxial?)⁴; the remaining axial musculature has become attached to the body wall.

The slender slips of muscle may reposition the skin; they do not transmit the propulsive forces which pass in static contact from column to skin to tunnel wall. The reinforcement of the wall may protect the visceral contents in a short-ribbed, elongate burrower.

Gaymer argued that the caecilian morphology was shaped by the need for internal curvature. Actually the separation between axial and body wall muscles maintains the patency of the visceral cavity by muscular rather than by skeletal elements.

I thank M. Wake and C. O. da C. Diefenbach for comments and the US National Science Foundation for financial support.

C. GANS

Department of Zoology,
University of Michigan,
Ann Arbor, Michigan 48104

Received November 21, 1972; revised January 22, 1973.

¹ Gaymer, R., *Nature*, **234**, 150 (1971).

² Gray, J., *Animal Locomotion* (Weidenfeld and Nicolson, London, 1968).

³ Gans, C., *Amer. Zool.*, **2**, 167 (1962).

⁴ von Schnurbein, A. F., *Gegenbaurs Morph. Jahrb.*, **75**, 315 (1935).

⁵ Gans, C., *Endeavour* (1973).

⁶ Smith, M., *Reptiles: Fauna of British India* (Taylor and Francis, London, 1943).

⁷ Buffa, P., *Atti Accad. Sci. Veneto-trentino-istriana*, **1**, 145 (1904).

⁸ Kaiser, P., *Zool. Anz.*, **154**, 61 (1955).

Tropical Role of Bacteria in the Ecosystem of the Coral Reef

THE factors responsible for the high productivity of coral reefs in oligotrophic tropical waters are still not understood^{1,2}. The relative importance of the predatory and symbiotically herbivorous nutrition of corals is not clear³, and other feeding

microscopy on stained membrane filters. The rates of bacterial production and bacterial destruction and of photosyntheses of phytoplankton and phytobenthos and a quantitative study of the feeding of aquatic animals with bacteria have been made using carbon-14⁸⁻¹¹. Some of my main results are summarized in Table 1 as quantitative estimates of the microflora in the water, bottom sediments and epibiotic layers covering dead corals.

The biomass and the production of bacteria are of the same range as those in a eutrophic or a mesotrophic lake¹², and several tens or even hundred times more than that in the pelagic regions of the ocean. The rate of destruction of organic matter usually exceeds the rate of primary production in the surface layers of water and sediments. The organic matter of microbial cells itself accounts for about 2–5% of the total organic matter of the reef sediments. The daily rate of bacterial production in sediments is usually around 30–60% of the photosynthetic production of phytobenthos and results in a daily production of raw bacterial biomass in the reef sediments of about 5–15 g/m² of the bottom surface.

The high rate of bacterial production may provide a significant part of the food of the very rich fauna of the coral reefs, which includes the filtering and the sand and detritus-eating animals. The latter groups include a part of the coral fishes. The experiments with ¹⁴C-labelled bacteria as a food showed that most of the common reef filter feeders can feed on bacterial plankton at concentrations equal to those in the lagoon water (Fig. 1). The consumption of bacterioplankton by crude filter feeders such as tunicates, oysters, and crustaceans is facilitated

Table 1 Quantitative Estimates of Bacterial Biomass, and Daily Values of Production of Bacteria, Photosynthetic Production and Destruction* in Coral Communities

Sampling area	Type of samples	Bacteria Biomass (B)	Bacteria Production (P)	Photo- synthesis (ph)	Destruction (D)	P/B	D/ph
Open ocean, north trade wind current	Bottom sediment, red clay	1.0	0.032	—	0.08	0.03	—
	Surface water	1.7	2.6	0.51	6.9	1.6	13.6
Fanning Atoll, Line Island	Coral sand	91	22.6	38	56	0.25	1.5
Great Barrier Reef, close to Heron Island	Coral sand	42.5	27.6	61	69	0.61	0.85
	Water over the reef	41	20.2	67	53	0.49	0.79
Majura Atoll, Marshall Islands	Dusty fine sediment among dead corals	88	35.2	74	100	0.40	1.4
	Epibiotic layer on the dead corals	28	14.3	610	41	0.51	0.06
	Coral sand	21	7.2	20	20	0.34	1.0
	Water over the reef	19	7.5	4.1	21	0.39	5.1
Kaneoche Bay, Oahu Island, Hawaii	Coral sand	65	17.2	7.1	49	0.26	6.7
	Seston over the surface of dead corals	147	73.0	96	207	0.50	2.2
	Water over the reef	43	28.0	36.3	79	0.67	2.2
Butaritari Atoll, Gilbert Islands	Water near the shore	170	41.5	37	110	0.24	3.0
	Water in the centre of lagoon	79	24	17	64	0.30	3.7

* mg C l.⁻¹ of raw sediment, or mg C m.⁻³ of water.

Variation range between parallel duplicates: primary production 20%, microbial production in surface samples and shallow sediments 15%, deep sea samples 50%.

mechanisms, filtering and osmotic nutrition^{4,5}, have not been studied sufficiently. Furthermore, very little is known about the microbial population of the reef ecosystems. Di Salvo⁶ showed that reefs support an abundant and active microflora which participates in nutrient regeneration and serves as food for filter feeders including some corals⁷, but there are no quantitative data relating to the biomass and production of microflora on reefs.

I collected data at several atolls in the Pacific Ocean during 1968–1970. Microbial biomass has been determined by direct

by the presence of about 20–30% of the total bacterioplankton in aggregates larger than 5 µm¹¹. Sand and seston eaters also consume and assimilate labelled bacteria in the sediments (Fig. 2).

Several series of experiments with corals were also carried out. The common corals (*Porites*, *Pavona*, *Hydnophora*, *Montipora*, *Fungia*, *Porcillopora* sp.) were fed with the labelled natural bacterioplankton and seston, in which the bacterial population was labelled with ¹⁴C. The evolution of labelled metabolic CO₂ by corals previously fed with labelled bacterio-

plankton, and then transferred into clean unlabelled seawater, was accepted as a measure of the intensity of feeding and digestion of labelled bacteria. All the corals tested were able to feed and digest the bacterial plankton (Fig. 2, Table 2) at concentrations close to that in the lagoon (Fig. 1). My data confirm Di Salvo's⁷ results and indicate the importance of careful studies of the filter feeding coelenterates.

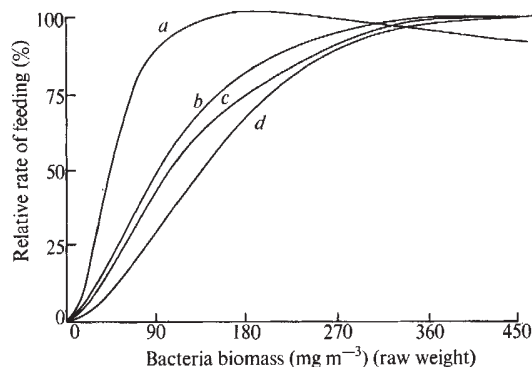


Fig. 1 Dependence of feeding rate upon the concentration of bacterioplankton for some common reef filtering animals. *a*, Sponge *Toxadocea*; *b*, veligers; *c*, oyster *Grossostrea*; *d*, coral *Pocillopora*.

Sections made off the lagoons of atolls and off barrier reefs into the open sea showed that the influence of the eutrophic coral communities upon the productivity of the surrounding blue oligotrophic tropical waters usually detected only in the shelf zone at a distance of 10–15 km from the reef (Fig. 3).

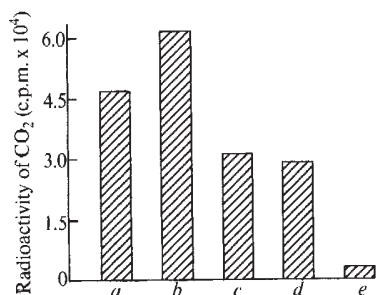


Fig. 2 The evolution of labelled CO_2 (in c.p.m. $\times 10^4$) by corals after exposure in seawater with ^{14}C -labelled bacterioplankton, and 2 h subsequent exposure in unlabelled water. The specific activity of the experiment was 35–40 g. *a*, *Pocillopora damicornis*; *b*, *Montipora verrucosa*; *c*, *Porites compressa*; *d*, *Fungia scutaria*; *e*, control (formalin).

These data demonstrate the important role of the bacterial population in the metabolism and productivity of the reef ecosystem. Its "internal" function is that of nutrient regeneration, and of the production of particulate protein food at the expense of the energy of mineralization of dead organic material. The "external" function of the bacterial population of coral reefs may lie in the consumption and accumulation of external energy, the energy of the organic matter contained in the oceanic waters passing over the reef. Surface oligotrophic tropical waters contain labile organic matter¹¹, which is mainly dissolved, and can be consumed by the rich epibiotic microflora which covers great areas of the surfaces of reefs, dead corals and sediments¹³. Further studies may help to evaluate the

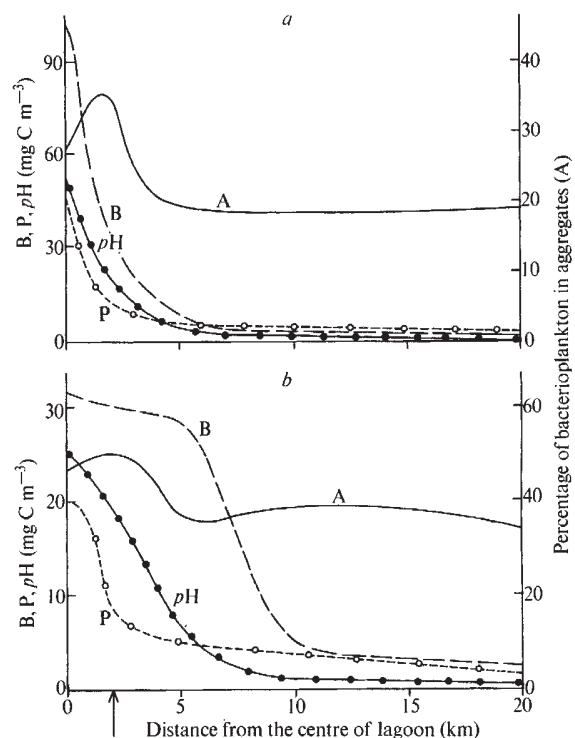


Fig. 3 The changes of biomass (B), daily production of bacteria (P) and photosynthesis (ph), % of bacterioplankton in aggregates (A) at two cross sections. *a*, From the centre of the lagoon at Tarawa atoll; *b*, off Heron Island (Great Barrier Reef) to the open sea. The arrow shows the break in the outer reef.

sources of the extra organic matter necessary to cover the excess of destruction over primary production in the reef communities, and to solve the enigma of the high productivity of these most interesting marine ecosystems.

Table 2 Daily Ration* and the Ratio % of Assimilated to Consumed Food † Determined by Feeding Common Reef Animals with Bacteria

Animal	R (%)	C (%)
Gastropod veligers	51.0	61
Hydroid, <i>Pennaria tiarella</i>	43.5	74
Annelid, <i>Serpullidae</i> sp.	7.8	73
Coral, <i>Pocillopora damicornis</i>	5.5	76
Coral, <i>Montipora verrucosa</i>	5.8	82
Sponge, <i>Toxadocea violacea</i>	3.4	82
Tunicate, <i>Ascidia nigra</i>	1.6	83
Holothurian, <i>Ophiodesoma spectabilis</i>	10.4	22
Gastropod, <i>Nerita picea</i>	9.4	20
Lamellibranch, <i>Grossostrea gigas</i>	2.2	68

* As % of the total body organic matter (R).

† Conversion, C.

I thank Drs E. Kreps, P. Helfrich, M. Doty, and K. Gundersen for their help and Dr F. R. Harden Jones who revised the manuscript.

Y. I. SOROKIN

Institute of Biology of Inland Waters,
Borok, Jaroslavl, USSR

Received September 7, 1972.

- ¹ Odum, H., and Odum, E., *Ecol. Monogr.*, **25**, 291 (1955).
- ² Johannes, R. E., Coles, S. L., and Kuenzel, N. T., *Limnol. Oceanogr.*, **15**, 579 (1970).
- ³ Yonge, C. M., *Adv. Mar. Biol.*, **3**, 209 (1963).
- ⁴ Roushdy, H. M., and Hansen, V. K., *Nature*, **190**, 649 (1960).
- ⁵ Stephens, G. C., *Science*, **131**, 1532 (1960).
- ⁶ Di Salvo, L. H., *Can. J. Microbiol.*, **17**, 1091 (1971).
- ⁷ Di Salvo, L. H., and Gundersen, K., *Can. J. Microbiol.*, **17**, 1081 (1971).
- ⁸ Romanenko, V. I., *Microbiologia*, **33**, 679 (in Russian) (1964).
- ⁹ Romanenko, V. I., *Microbiologia*, **34**, 391 (in Russian) (1965).
- ¹⁰ Sorokin, Yu. I., *Mitt. Int. Ass. Theor. Appl. Limnol. Mitt. Int. Verein Theor. Angew. Limnol.*, **18**, 41 (1968).
- ¹¹ Sorokin, Yu. I., *Int. Rev. Hydrobiol.*, **56**, 48 (1971).
- ¹² Kuznetsov, S. I., *Microflora of Lakes and its Geochemical Activity*, 486 (in Russian) (Ac. Sci. USSR, Leningrad, 1969).
- ¹³ McIntyre, A. D., Munro, A. L. S., and Steele, T. H., in *Marine Food Chains*, 552 (University of Aarhus, Denmark, 1968).

Inhibition of Monoamine Oxidase by the Pesticide Chlordimeform and Related Compounds

CHLORDIMEFORM is a formamidine acaricide and insecticide used to control phytophagous mites, cattle ticks and certain lepidopterous insects. Because of its unique spectrum of biological activity with regard to selectivity, chlordimeform may be the forerunner of a new class of agricultural chemicals. Although its mechanism of action is unknown, Knowles and Roulston¹, studying the action of chlordimeform and related compounds on the southern cattle tick, suggested that inhibition of monoamine oxidase (MAO) could be involved. This hypothesis was based on observations of ticks poisoned with formamidine compounds and on the known ability of amidine compounds, such as propamidine and pentamidine, to inhibit MAO from mammals². We have observed that symptoms manifested by rats poisoned by chlordimeform and demethylchlordimeform are similar to those elicited by sympathomimetic agents including known MAO inhibitors (unpublished results of F. R. Johannsen and C. O. K.). Thus, it seemed appropriate to examine chlordimeform and related compounds as potential inhibitors of rat liver MAO *in vitro*.

For preparation of the enzyme, freshly dissected rat liver was rinsed with Ringer solution, and a 20% homogenate was prepared in distilled water, filtered through cheesecloth and centrifuged at 500g for 10 min. The supernatant was used as the MAO source. For MAO assay a standard incubation mixture contained 0.2 ml. of the enzyme preparation, 0.3 μ mol of kynuramine, and 2.8 ml. of 0.05 M phosphate buffer, pH 7.4. Deamination of kynuramine was followed by a decrease in absorbency at 360 nm at 27° C as described by Weissbach *et al.*³. The inhibitory potency of chlordimeform and related compounds was examined by adding solutions of these compounds at various concentrations to the enzyme 30 min before addition of the substrate followed by the usual assay. All experiments were repeated at least twice, and the data were averaged. Data are expressed as I_{50} values which represent the concentration of the compound in mol/l. giving 50% inhibition of MAO activity.

The results of inhibition of MAO activity from rat liver by chlordimeform and related compounds are given in Table 1. When compared with iproniazid and tranylcypromine, two classical MAO inhibitors, chlordimeform (I) was a moderately strong inhibitor. However, demethylchlordimeform (II), a chlordimeform metabolite previously isolated from mammals,

insects, spider mites⁴, and southern cattle ticks (unpublished results of C. A. Schuntner and C. O. K.), was even more potent than chlordimeform. This is especially interesting in view of the suggestion¹ that demethylchlordimeform is probably the actual toxicant in chlordimeform-poisoned southern cattle

Table 1 Inhibition of Rat Liver Monoamine Oxidase Activity by Chlordimeform and Related Compounds

Compound	Structure	$I_{50}(M)$
(I) Chlordimeform		1.4×10^{-5}
(II) Demethylchlordimeform		4.7×10^{-6}
(III) HOKKO-20013		1.1×10^{-5}
(IV) C-10405		2.2×10^{-5}
(V) BTS-27271		2.7×10^{-5}
(VI) C-17294		5.9×10^{-4}
(VII) C-17296		5.0×10^{-4}
(VIII) C-14640		7.2×10^{-4}
(IX) U-36059		6.6×10^{-7}
(X) Iproniazid		6.3×10^{-6}
(XI) Tranylcypromine		5.8×10^{-7}

ticks. Table 1 suggests that in the substituted aryl alkylformamidines examined maximum inhibitory potency was associated with those compounds containing the 4-chloro-*o*-tolyl moiety. Thus, compounds I, II and III were more active MAO inhibitors than were compounds IV, V, VI, VII and VIII. U-36059 (IX),

an acaricide structurally related to the formamidines, was the most potent compound tested; its anti-MAO activity compared favourably with that of tranlylcypromine.

We have shown that chlordimeform and several related compounds are potent inhibitors of the deamination of kynuramine by rat liver MAO *in vitro*. Although it is possible that these compounds will also interfere with other enzyme systems, it seems plausible to suggest that a causal relationship exists between the toxic action of these formamidine compounds and interference with the MAO-biogenic amine system. This is the first observation of inhibition of MAO by insecticides and acaricides.

SHAWKY A. AZIZ
CHARLES O. KNOWLES

Department of Entomology,
University of Missouri,
Columbia,
Missouri 65201

Received December 21, 1972.

- ¹ Knowles, C. O., and Roulston, W. J., *J. Austral. Entomol. Soc.*, **11**, 349 (1972).
- ² Blaschko, H., and Duthie, R., *Biochem. J.*, **39**, 347 (1945).
- ³ Weissbach, H., Smith, T. E., Daly, J. W., Witkop, B., and Udenfriend, S., *J. Biol. Chem.*, **235**, 1160 (1960).
- ⁴ Knowles, C. O., Ahmad, S., and Shrivastava, S. P., in *Proc. Second IUPAC Intern. Cong. Pesticide Chem.*, **1**, Insecticides, 77 (Gordon and Breach, New York, 1971).

How Much Food Does Man Require?

WE believe that the energy requirements of man and his balance of intake and expenditure are not known. Paradoxically, we conclude this from results of the increasingly sophisticated studies of food intake and energy expenditure which show that in any group of twenty or more subjects, with similar attributes and activities, food intake can vary as much as two-fold¹⁻⁵. In those surveys where both intake and expenditure are measured, there is often good agreement between the two estimates for the average of the group, but usually very large discrepancies between individual intake and individual expenditure. The results of careful studies in a number of countries suggest that some people, perhaps through some mechanism of adaptation, are able to be healthy and active on energy intakes which, by current standards, would be regarded as inadequate. On the other hand, there are also studies in which subjects have been given large quantities of additional food with little or no increase in body weight^{6,7}. In contrast, there are the difficulties experienced by the obese in reducing body weight in spite of a drastic reduction of food intake, and the well recognized fact that many fat people eat no more, and sometimes less, than those who are not obese. These observations underline the extent of our ignorance about the mechanisms by which energy balance is maintained.

When the energy requirements of large populations are calculated, using the currently available international standards prepared by the Food and Agriculture Organisation and the World Health Organisation, then it may be, and often is, concluded that a large proportion of the world's population is undernourished; present standards put this proportion at about 70%. This estimate is based on a careful examination of all available information; such estimates are essential for governmental planning of, for example, food production. Some of us have assisted in this preparation and suggest these

estimates with all their faults are the best that can be achieved on present knowledge.

It is possible, however, that the 30% of the world's population who have an "adequate" intake are really eating too much and that an unknown proportion of the rest are not undernourished. Our present information does not provide a satisfactory basis for more accurate estimates, as the methods commonly used are not precise enough and cannot be validated. Furthermore, the measurement of normal daily food intake and energy expenditure poses many technical and logistic problems, requires large teams of skilled staff and is expensive; hence so far only small populations of individual men and women have been studied. It is difficult to finance such work, and there are relatively few surveys in which individual food intake has been measured with acceptable levels of accuracy.

These are some of the reasons that prompt us to assert that we do not know how much food man requires. If there is merit in this assertion, and no doubt many will question it, we would stress that to determine man's energy need with more precision should be regarded as a major and urgent task. We believe that appropriate methods can be developed; for example, if a calorimeter suitable for man was available it would be possible to calibrate the various ways by which food intake, energy expenditure and energy balance are assessed. Calorimeters for domestic animals have been constructed, but none suitable for man exists today. This work would not be easy or cheap, but we suggest the social and political advantages of obtaining accurate answers could be immense.

J. V. G. A. DURNIN

Institute of Physiology,
Glasgow

O. G. EDHOLM

Division of Human Physiology,
National Institute for Medical Research,
London

D. S. MILLER

Department of Nutrition,
Queen Elizabeth College,
London

J. C. WATERLOW

Department of Human Nutrition,
London School of Hygiene and Tropical Medicine,
London

Received December 15, 1972.

- ¹ Widdowson, E. M., *Spec. Rep. Ser. Med. Res. Coun. No. 257* (1947).
- ² Widdowson, E. M., *Proc. Nutr. Soc.*, **21**, 121 (1962).
- ³ Rose, G. A., and Williams, R. T., *Brit. J. Nutr.*, **15**, 1 (1961).
- ⁴ Ashworth, A., *Brit. J. Nutr.*, **22**, 341 (1968).
- ⁵ Wynn-Jones, C., Atkinson, S. J., and Nicolas, P., *Proc. Nutr. Soc.*, **31**, 83A (1972).
- ⁶ Miller, D. S., and Mumford, P. M., *Amer. J. Clin. Nutr.*, **20**, 1212 (1967).
- ⁷ Miller, D. S., Mumford, P. M., and Stock, M. J., *Amer. J. Clin. Nutr.*, **20**, 1223 (1967).

BOOK REVIEWS

Vive la Différence

Gender Differences: Their Ontogeny and Significance. Edited by C. Ounsted and David C. Taylor. Pp. x+273. (Churchill Livingstone: Edinburgh and London, 1972.) £4.50.

A SCOTTISH medical professor once remarked that it was a pity there were not more women doctors; they are, he said, better people than the men. It was easy to discern his meaning, difficult to imagine a way of expressing it objectively. Most of this useful book is at the other extreme: it gives, systematically, the hard facts about the subtler differences between the sexes. The contents of most chapters (and the drab production) suggest that something like a textbook was aimed at.

C. O. Carter gives a helpful, if apparently hurriedly written, review of sex-influence ("sex-limitation") and sex-linkage. His account of the interaction of genotype and environment in the aetiology of pyloric stenosis is especially instructive. P. E. Polani puts the reader up-to-date on sex-chromosome anomalies. I. N. Mensh and John Bancroft contribute overlapping chapters on the role of the social environment in the determination of "gender identity"—that is, what sex one feels oneself to be. That of Mensh, which consists largely of quotations from the work of R. J. Stoller and his colleagues, is particularly interesting. It begins with the selected young men of Madagascar, Tahiti and other regions who were raised as girls and considered themselves completely feminine; it ends with a pessimistic discussion of the prognosis for treatment of trans-sexualism; in between there is some notable evidence of the phobias from which many (American) physicians suffer in dealing with these "disorders".

Corinne Hutt provides a long, wide-ranging review with much valuable documentation on effects of separation from the mother, other aspects of social behaviour, and the development of intellectual abilities. There is a strenuous effort to be critical; but comparisons between men and women, like those between races and social classes, are technically as well as politi-

cally hazardous. For example, Dr Hutt does not consistently distinguish between saying that women are different from men and saying that they are superior or inferior. The importance of this emerges in a report by Hugh Fairweather and John Hutt, on studies of reaction time at various ages. Is it better to have a short reaction time or a long one? Presumably, for a given task, there is an optimum; and the useful thing to know is the distributions of scores about this figure.

Detailed analysis of sex differences leads to few simple conclusions. A. W. H. Buffery and J. H. Gray examine the ontogeny of spatial and linguistic skills. Girls and women usually score lower than boys and men in manipulation of spatial relationships, but not in other visual tasks. Girls are notoriously more fluent speakers than boys, yet are often found to have smaller vocabularies. But these statements about averages say nothing on the distributions of the scores; and the existence of a big overlap between distributions may be more important than the differences between the means. There are other complications: in one enquiry on the perception of space, female scores were unimodal, but the male scores were bimodal.

Often, indeed, differences between means, even if enormously "significant" statistically, are of trifling value for practical purposes. Christopher Ounsted and David Taylor devote an important but not entirely lucid chapter mainly to the clinical implications, actual or hypothetical, of the well-known fact that populations of men tend to vary more than those of women. Hence we have the superiority of men over women in the production of idiots. They discuss the incidence, in relation to age and sex, of a number of neurological disorders, infectious illnesses and other diseases; and they attempt an interpretation of the figures in terms of the increased variance imposed by the presence of a Y-chromosome. Their chapter is appropriately preceded by two of a less speculative character. Margaret Ounsted reviews intra-uterine growth.

And Martin Vessey writes on gender differences in the epidemiology of non-neurological disease; this chapter is an admirably lucid account of carcinoma of the lung, carcinoma of the stomach and ischaemic heart disease. The incidence of these conditions in relation to sex and age presents a fascinating, if sometimes dismaying, picture.

Knowledge in this field is growing quickly, and I hope the editors will be willing to organize a second edition after a few years. If so, the methodology could be tightened up. This applies especially to the uses of analogies with other species; and, still more, to the treatment of the interaction between heredity and environment, over which several contributors stumble. Dr Carter, instead of ending his chapter abruptly, could have helped both his colleagues and his readers by concluding with a clear explanation of how to make valid statements on this subject. Minor needs include a summary for every chapter, an author index, and uniform treatment of tabular material: statements of means should always be accompanied by the number of observations in each class and a measure of the variance: all measures should be in metric; and abbreviations should conform to international standards.

This book may not tell us whether women are "better people" than men, but it gives the type of authentic information on which the decisions of governments, teachers, doctors and others should be based.

S. A. BARNETT

Future of Rubber

Natural Rubber and the Synthetics. By P. W. Allen. Pp. 255. (Crosby Lockwood: London, December 1972.) £4.95.

THIS book is essentially the history of the fight of natural rubber to retain its position as a major rubber in competition with the synthetics. It contains a broad analysis of the structures of the two very different indus-

tries, together with a technical comparison of the behaviour of both natural and synthetic products in the modern rubber manufacturing industry.

Although the author, in spite of his natural rubber bias, expects that natural rubber will increasingly be replaced by synthetics where price makes them competitive, he can find no evidence to suggest that the production of natural rubber will decline in absolute terms. Indeed, natural rubber is still the preferred polymer for many high performance applications (for example, truck tyre carcasses) and even in the short term there is a relatively untapped potential for technological improvement which results from the late application of science-based research in this industry. Indeed, on the basis of the evidence presented in this book, the synthetic rubber industry has had a beneficial stimulatory effect on the natural rubber industry. The latter has belatedly learned the need for effective technical specification and efficient presentation of its product.

In the long term, too, natural rubber probably has the greatest potential for cost reduction. Effective steps have already been taken to improve the profitability of natural rubber by the selection of high yielding clones and by the development in recent years of yield stimulants. At the same time that the synthetic rubber industry is faced with the prospect of higher raw materials costs, the yield improvement in natural rubber has tipped the economic scales slightly in favour of the natural product.

This book is written in a style which is easy to read and it does not assume specialized technological or economic knowledge. Perhaps the most interesting chapters are those concerned with the economic structures of the two industries. These do, however, suffer from the deficiency that hard facts are not always available. Nevertheless, the book is a valuable starting point for students of technological economics who are concerned with the future of the rubber industries.

GERALD SCOTT

Quantum Field Maths

Mathematics of Contemporary Physics. Edited by R. F. Streater. Pp. xi+274. (Academic: New York and London, December 1972.) £6; \$17.50.

ONE important theoretical problem in high energy physics today is to obtain a better understanding of quantum field theory. Many models claiming to be good descriptions of the properties of elementary particles are still languishing for want of powerful enough field theoretic techniques to calculate their detailed predictions. Because of this pressure there is a great

deal of activity at present attempting to lay a firm mathematical foundation to quantized systems with infinitely many degrees of freedom.

The volume under review comprises the contents of the major lectures given at an instructional conference at Bedford College in September 1971 on this theme. As such, the title is misleading, for there are many areas of mathematics of contemporary physics other than those reported on. As a collected set of lectures in the more select area of Hilbert spaces, operators thereon and operator algebras, and their applications to problems of quantum field theory and quantum statistical mechanics, the volume is a very valuable contribution to those who wish to enter this field or even to find out in general what success is being achieved.

The lectures open with a useful introduction by R. Haag to the basic ideas of quantum field theory, and continue with a very valuable and reasonably self-contained course by B. Simon on functional analysis of operators on a Hilbert space and related topics. There is then a detailed discussion from J. Glimm and A. Jaffe of the existence problem for boson quantum field models, the basic properties of two or three dimensional self-interacting models being discussed, including how to deal with infinite counter-terms. There is then a description of the axiomatic approach to statistical mechanics by N. Hugenholtz, a brief guide to the rigorous theory of quantum scattering by K. Hepp, a presentation of Segal's theory of linear fields by P. Bongaarts, an analysis of the free boson gas by J. Lewis and of the Lorentz transformation properties of self-interacting boson fields by A. Klein. The volume concludes with a course of algebras with quasilocal structure and factorizable representations.

The majority of the courses are at a high level, and would undoubtedly be difficult for the casual reader. For the research worker, be he mathematician or theoretical physicist, who wants to perform research in quantum field theory or in operator algebras with relevance to physics, this volume allows him to come to grips with the most valuable recent work in the field. As such it will be indispensable for the second year research student; it should also prove valuable to the more advanced research worker.

The only defect of the book is that there is little general discussion of progress, of whether the problem posed by a realistic quantum field theory, such as that of electrodynamics, is being approached. But then it may be up to the readers of the book to make the contributions which will achieve that. Undoubtedly there is much yet to be done in this area. J. G. TAYLOR

Drug Dependence

Chemical and Biological Aspects of Drug Dependence. Edited by S. J. Mule and Henry Brill. Pp. 561. (The Chemical Rubber Company: Cleveland, Ohio, 1972.)

MANY drugs exist which have little in common except their ability to produce dependence—the word now favoured throughout the world for addiction—which because of its moral connotations inhibits rational and scientific investigation of the social and medical problems which arise from the ability of such drugs to affect mental processes. Nevertheless, it is the existence of these problems that has led to the upsurge in concern about the chemical and biological properties of the psychoactive substances amongst which are some of the oldest and newest of the drugs known to man. The present volume is a comprehensive compendium of information about drugs currently known to produce dependence in man or animals. It is written in the form of a series of twenty-five authoritative and virtually self-contained reviews loosely grouped together. References to the literature are copious but not exhaustive and in the main are to literature that has appeared within the past five years up to and including 1971.

The five sub-sections into which the book is divided deal with the social and medical significance and biological characteristics of drug dependence; criteria for its evaluation; the chemistry of the drugs involved; their physiological and pharmacological properties and, finally, the biochemical, metabolic and endocrinological consequences of their use and abuse.

Perhaps inevitably there is considerable overlap between chapters and information about individual drugs is scattered diffusely throughout the volume, which makes the absence of an index the more lamentable. What might be considered the forensic and clinical aspects of drug dependence are rigorously excluded. Surprisingly, this detracts comparatively little from this book, which fills an important gap in our knowledge. Nevertheless, it robs the otherwise excellent chapter on the detection and identification of drugs of dependence of any discussion of the usefulness, or otherwise, of the enormous number of urinalyses currently undertaken in clinical laboratories in this country and elsewhere.

The aim of the publishers to provide "highly authoritative reference works dealing with those subjects for which there exists an urgent necessity for published information . . ." has undoubtedly been fulfilled. Such aims do not lend themselves well to the development of literary style, but for anyone concerned with the problems

of drug dependence, however remotely, this book will prove invaluable as one that brings together information otherwise only to be found scattered throughout the literature of many unrelated academic disciplines.

VINCENT MARKS

Mass Spectrometry

Principles of Organic Mass Spectrometry. By Dudley H. Williams and Ian Howe. Pp. 245. (McGraw-Hill: London and New York, November 1972.) £5.

THIS is a useful, indeed a very valuable, little book which outlines most of the aspects of modern mass spectrometry currently being investigated and presumably represents a fairly elementary series of university lectures. It comprises a series of comprehensive if rather condensed chapters upon the mass spectrometer, ionization and energy transfer, metastable ions, the quasi-equilibrium theory, energetics of ion decomposition, the relationship between fragmentation pattern and structure, isotopic labelling, collision processes, field ionization, ion structures, gas chromatography-mass spectrometry and the use of the computer in mass spectrometry. Each chapter concludes with a succinct list of relevant reviews and cognate papers useful for further reading. The book concludes with a useful index.

There are some trivial points raised in the text, and the few following items invite more serious criticism. Firstly, the attention paid to S.I. units is uneven, suggesting that one author may be less than enthusiastic about this new innovation. It may have been desirable to have included a table equating the newer units with the traditional ones of torr, kilocalories and so on. The second is the needless confusion occasioned in the mind of a beginner in this field by failing to call attention to the difference between H when the magnet is held steady while the voltage varies, and B —the magnetic induction—when the magnetic field is varying (page 4, 1's 1 and 2) rather than the voltage. This book also commits the now standard error of misquoting Stevenson's Rule (pages 94–97).

A further source of regret is occasioned by the practice of including valuable if not vital information in the form of footnotes or commentaries upon the tables. Much of the material so conveyed is often vital to a logically presented argument which, therefore, loses something of its power.

This book is well produced and is in the best traditions of scholarship. It is printed in a very clear legible type and is accompanied with a set of excellent explanatory diagrams. Both the publishers and the authors are to be

congratulated upon the production of this book.

ROWLAND I. REED

Fossil Manual

Palaeontology and Geological Time. Prepared by the Course Team. (The Open University. Science: A Second Level Course.) Pp. 64. (The Open University: Bletchley, Buckinghamshire, 1972.) £1.10.

AUTHORS of this type of remote-control laboratory manual carry an enormous responsibility to their followers because they will have limited opportunity to rectify misunderstandings and textual errors. Thus, though one would wish to welcome such a venture in principle, one cannot help being disappointed at the outcome. This is a pity: a great opportunity has been missed for raising standards of geology teaching in many secondary schools and first-year university classes. Instead, an attractively designed, seemingly cheap, 63-page booklet has appeared on the market, which will doubtless sell well as a pre-examination crib despite the following glaring deficiencies.

The adult user will be deprived of basic facts necessary to comprehending the study as a result of over-simplification. The younger user, or by-product, on the other hand, will have absorbed a doctrinaire approach which, when based on misleading statements, will undermine his receptiveness for subsequent professional training. Thus, for all its good points in bringing science to the man in the street, this volume sets a dangerous educational precedent; when it could so easily have contributed substantially to an advancement of the science at no extra cost.

The text is arranged in a series of introductory paragraphs copiously illustrated with classical diagrams, which are followed by outline quizzes reminiscent of a weekly woman's magazine. The desired answers to these self-assessment tests are at the back of the book, but unfortunately they do not explain the reasons for eliminating alternative answers (for example, SAQ3 c and f). Thus, those who have erred, and even may have been technically correct, remain lost.

The best aspects of the volume are its glossary and its attempt to cover the broad field of palaeontology. The weaknesses lie in its keys, particularly Table 2, which is biologically misleading; elementary scientifico-grammatical inaccuracies in technical diagrams (SAQ4B6; SAQ6D4; SAQ9F7); clumsy annotations, bar scales are never used to indicate sizes; naïve popularizations of complex themes, for example, coral clocks and intraspecific variation. The unfortunate adoption of North American examples, where European alternatives are available, is surprising.

But the worst aspect of all is the treatment of the "Fossilization Process", which is wholly inadequate.

This is a neo-classical exercise book, complete with errors. In failing to delimit the limitations of their keys the authors show little appreciation of the biological and consequent mineralogical background of palaeontology; thus the consequences reveal themselves as a shower of random facts. Thus the volume is best suited to redeploying the energies of 12-year-old boys and their fathers on wet holidays.

J. A. E. B. HUBBARD

Parapsychology

Psychical Phenomena and the Physical World. By Charles McCreery. Pp. 158. (Hamish Hamilton: London, January 1973.) £2.30.

MR MCCREERY'S book, written with clarity and objectivity, is the fourth volume of the Proceedings of the Institute of Psychophysical Research, and continues the study of lucid dreams (those in which the subject knows he is dreaming), ecsomatic experiences and the philosophical and scientific questions raised by these and considered in earlier proceedings. The book is divided into a first part dealing with philosophical questions raised by such experiences, and a second containing empirical material.

The former considers lucid dreams, ecsomatic experiences, apparitions, materializations and psychokinesis. It discusses such questions as the criteria whereby we may distinguish the waking from the sleeping state; perception in out-of-the-body experiences and the difficulty of distinguishing between physically seeing the world and having hallucinatory perceptions which correspond to it; the problem of establishing complete satisfactory standards of distinction between apparitions and "real" people; the nature of materialization, and the differences between materialization, apparitions and physical objects; and the fact that, though psychokinesis seems inconceivable, yet we cannot explain how we move objects physically, nor do we even know how we control our limbs.

The empirical section of the book begins with an account and analysis of a number of extra-sensory perception card-guessing tests, investigating the capabilities of eldest, only and younger children in family order. In the three most significant experiments eldest children did best, although other factors such as their socio-economic groupings affected the results. There follow studies of a lucid dreamer, the Marquis d'Hervy de Saint-Denys, born 1822, who published a detailed record of his dreams; the lucid dreams and ecsomatic experiences of "Subject E"; and some

miscellaneous cases of ecsomatic experiences containing common features such as the feeling of being "above" the physical body, autoscopia (the seeing of one's own body), the subject's emotional relationship to his physical body in the ecsomatic state, and the seeing of things, sometimes by "travelling", which subjects could not see in their physical bodies. The work ends with an account of experimental evidence of the fulfilment of a previous prediction of the author that subjects whose alpha-frequency was accelerated above their own pre-test baseline in electroencephalogram recordings did better in ESP tests than others.

The value of the book lies in its accumulation of case-material and the raising of questions suggested by this. Scientists, theologians, parapsychologists and psychical researchers will find it useful and stimulating.

D. CHRISTIE-MURRAY

Story of Immunology

The Science of Self: a Report on the New Immunology. By David Wilson. Pp. xii+346. (Longman: London, June 1972.) £2.75.

THIS exemplary text on "the new immunology" is good enough to be read as a primer by beginners in the field—by PhD starters, for example, by surgeons turning their hand to immunological research, or by anyone whether in the health business or not anxious to get a synoptic view of the most exciting branch in modern medical biology.

For Wilson the new immunology grows out of the study of self-recognition. The special virtue of this approach is that it makes it possible to embrace cancer immunity and auto-immunity within one comprehensive narrative. From the standpoint of the history of ideas the most interesting thing about immunology is how little progress it made between the great days of Behring, Bordet and Koch, and its modern developments by chemists, zoologists, molecular biologists and experimental surgeons. The reason is that over the whole of the period during which it was in the doldrums immunology was running along what may be called "Rothschild" lines, that is, orientated strictly towards practical usefulness and material advantage—antiserum, vaccines and the like—without sufficient attention to chemical and biological fundamentals. In this conception of the way science ought to develop the kind of skin-grafting work that played such a large part in the growth of the new immunology might easily have been dismissed as laboratory play.

The New Immunology is a very good story and David Wilson has both the

technical knowledge and the literary skill to make it clear that it is so.

P. B. MEDAWAR

Harvesting the Waters

Aquaculture: The Farming and Husbandry of Freshwater and Marine Organisms. By John E. Bardach, John H. Ryther and William O. McLarney. Pp. xii+868. (Wiley: New York and London, November 1972.) £15.95.

THIS is an ambitious book devoted to a subject which is attracting increasing attention. Most of the important natural resources of fish and shellfish in the sea have now been discovered and exploited, and in areas such as the North Atlantic the amount of fishing effort deployed is already more than enough to harvest the maximum sustainable yield in an economic manner. On the land, development and pollution may seriously restrict freshwater fish production, while in the intervening estuaries and coastal fringes conditions have often sadly deteriorated as the result of industrialization. In such circumstances the possibilities of land-based culture systems for fish, molluscs and crustaceans, and of "farming" shallow coastal waters, are beginning to look increasingly attractive. Thus the book is very timely, particularly because of its very wide coverage. It is also very clearly written in plain English and well illustrated by clear photographs and line diagrams.

The plan of the book is straightforward. The first chapter establishes general principles and examines the economics of different culture systems. It is followed by no less than forty-two specialized chapters dealing successively, among many others, with carp, catfish, mullets, milkfish, *Tilapia*, eels, salmonids, sturgeon, pompano, yellowtail, marine flatfishes, shrimps, lobsters, freshwater crayfish, oysters, mussels and seaweeds. There is a useful appendix on pond siting and construction.

References are given separately after each chapter; the coverage varies, but is sometimes rather less than one might expect—eighteen references to the culture of freshwater salmonids seems a meagre guide to anyone wishing to consult the original papers. There is, moreover, no chapter on Atlantic salmon, although its Pacific cousins receive separate treatment in a very informative chapter of fifty-one pages.

In addition to references, the authors give after each chapter a list (with addresses) of those people interviewed or from whom they received personal communications. There are some unexpected gaps; to a European, for example, it is strange to find no Dutch references in the chapters on oyster and mussel culture. On the other hand,

the book is particularly informative about Japanese and Far Eastern practices which are not well described in easily accessible references. Another good feature is its insistence on looking realistically at the economic prospects of the culture systems described. All in all, this is a very useful book although, despite its length, the specialist must still seek out the original papers and must not assume that coverage is complete. He will, however, usually find some stimulating ideas.

H. A. COLE

Plant Viruses

Principles and Techniques in Plant Virology. Edited by Clarence I. Kado and Hari O. Agrawal. Pp. xv+688. (Van Nostrand Reinhold Company: New York and London, 1972.) £14.10.

THIS is a very useful book indeed, even though it can hardly be expected to fulfil the claim on the dust cover to include "in extensive detail both the principles and techniques of every aspect of plant virology". Besides control measures and nomenclature (which were omitted for stated reasons not necessarily valid) insect vectors other than aphids and leafhoppers, for example, are not considered.

The book will be helpful to research workers seeking information on recently developed techniques. These develop very rapidly, however, and I was not surprised to find a few places where information was already incomplete. For university lecturers and their students this book will fill a long-felt need.

It is divided into four parts. The first, which is called "Biology of Plant Viruses", comprises chapters on inoculation principles, inhibition, interference and acquired resistance, inclusion bodies, and (oddly placed) electron microscopy. The second deals with transmission by aphids, plant hoppers, mites, nematodes, and fungi, and through seed and pollen. The third part on "Isolation and Characterization" contains chapters on purification, preparative centrifugation, chromatographic purification, electrophoresis, isolation and properties of virus proteins, virus degradation and nucleic acid isolation, serology and analytical ultracentrifugation. The fourth part, called "Photobiology and Mutation", is more esoteric and less concerned with methodology.

Each chapter is authoritative, having been written by a research worker specializing in the subject, and reflects the outlook of the author. The standard of clarity, if not uniform, is certainly high. The editors must be congratulated on compiling a book which should be on hand for reference in all plant virus laboratories.

A. F. POSNETTE

CORRESPONDENCE

Dingle's Question

SIR,—Professor J. Ziman¹ and Mr G. F. R. Ellis² seem not to have read my "question", let alone answered it, though Ziman quotes it correctly. Neither of the events need be at either of the clocks concerned, so the statement, "the fastest working clock between any two events is one that travels between them by free fall", is futile.

Two observers-cum-clocks, A and B, are in uniform rectilinear relative motion. Each receives light from each of the same two events, occurring at any ascertainable positions at any times, and, *using the theory*, finds the times by his clock at which the events occurred. These times will be separated

by intervals, dt for one clock and dt' for the other, and these intervals, *according to the theory*, will be related by the Lorentz transformation, and so will be unequal. My question is: how does the theory indicate which clock gives the larger interval? If A has velocity 0 and B velocity v , the Lorentz transformation makes that clock A; if B has velocity 0 and A velocity v , it makes that clock B. Also, *according to the theory*, these assignments of velocity are equally valid. Hence, unless my question receives an acceptable answer, the theory requires each clock to give a larger interval between the same events (i.e. to work faster) than the other, which is impossible.

It is not a case of each clock appearing to work the more slowly to an

observer with the other. A and B observe only the events, not each other, and each may be ignorant of the other's existence. They can be brought together later, introduced, and asked to compare notes. What feature of the situation enables us to say, consistently with the theory, which set of clock-readings has yielded the larger interval between the events? Is there no one who will answer that question, or have the courage to admit the obvious fact that no answer is possible and therefore the theory must be false?

Yours faithfully,

HERBERT DINGLE

Purley,
Surrey

¹ Ziman, J., *Nature*, **241**, 143 (1973).

² Ellis, G. F. R., *Nature*, **242**, 143 (1973).

Obituary

Academician K. I. Skryabin

ACADEMICIAN KONSTANTIN IVANOVICH SKRYABIN, one of the leading Soviet specialists in the medical and veterinary sciences and virtually the founder of helminthological research in the Soviet Union, died on October 17, 1972, at the age of 93.

Skryabin was born on December 7, 1878, in St Petersburg. His father was a railway engineer, and his international family background gave the young Skryabin an unusually wide outlook on life.

After an unsettled early education Skryabin enrolled in the Yur'ev (now Tartu) Veterinary Institute in 1900. Here his work quickly attracted the attention of the Director of the Institute, Professor K. K. Raupakh, who in 1903 took Skryabin as one of a small party of specially privileged students allowed to attend the First All-Russian Veterinary Congress held in St Petersburg.

In 1905 he graduated from the Yur'ev Institute, and began his career as a veterinarian working in Central Asia. In 1912–14 he made a prolonged visit to western Europe, working at Königsberg and Neuchâtel Universities and the Alfort Veterinary School.

On his return to Russia he was appointed Head of Department of Veterinary Medicine and Zoohygiene

of the Statbutov Higher Agricultural Courses for Women (the equivalent of university courses for women at that time).

In 1917, he became Professor of Parasitology at the Don Veterinary Institute, in Novocherkassk, and thus was able to begin his intensive work on helminthology, a subject which had first attracted his attention during his time in Central Asia. In 1920 he became Head of the Helminthology Department of the All-Union Institute of Experimental Veterinary Medicine and Head of the Department of Parasitology of the Moscow Veterinary Institute, and from then onward held a number of high academic and teaching posts, culminating in his appointment in 1956 as Vice-President of the All-Union Lenin Academy of Agricultural Sciences, a position which he held until 1971. He became an Academician of the Soviet Academy of Sciences in 1944, and was subsequently elected an honorary member of the Academies of Bulgaria, Hungary, Czechoslovakia, East Germany, Yugoslavia, France, and of Scientific Societies in the USA, Great Britain, Belgium, West Germany and India.

Skryabin published more than 700 papers and monographs on various aspects of helminthology and parasitology including: *K kharakteristike gel'mintofauny domashnikh zhivotnykh Turkestana* (on the characteristics of

the helminthofauna of domestic animals of Turkestan—1916; his doctoral thesis); *Askariy i ikh znachenie v meditsine i veterinarii* (Ascarides and their significance in medicine and veterinary science—1925); *Trematody zhivotnykh i cheloveka* (Trematodes of animals and man, 20 volumes, 1947–62), *Osnovy nematologii* (Principles of nematology, 10 volumes, 1949–61), and his autobiography *Moya zhizn' v nauke* (My life in science—1969).

Skryabin devoted great efforts to developing the study of helminthology throughout the Soviet Union, founding helminthology departments in scientific institutions and universities in almost every republic of the USSR. In the early 1920s he also founded the three major helminthological bodies in the Soviet Union: the Helminthological Department of the State Institute of Experimental Veterinary Science (later transformed into the All-Union Institute of Helminthology, which bears his name), the Helminthology Department of the Moscow Tropical Institute, and the Helminthology Bureau of Moscow State University, which in 1942 became the Helminthology Laboratory of the Soviet Academy of Sciences, and of which Skryabin remained the head until his death.

He was extremely interested in promoting international cooperation in helminthological research, and it was on his initiative that commissions for

international research and practical work within the framework of the COMECON block were set up and the international journal *Gel'mintologiya* (Helminthology) was founded. He was also a co-editor of a number of Soviet publications on veterinary medicine and a member of the Editorial Board of *Meditsinskaya parazitologiya i parazitarnye bolezni* (Medical parasitology and parasitic diseases).

Skryabin took part in numerous All-Union and International Congresses on Helminthology. He also led more than 300 helminthological expeditions to all parts of the USSR. For his work, which won him in his lifetime the unofficial title of "Founder of Soviet Helminthology", his official recognitions include the Order of Lenin (6 times), the Order of the Red Banner of Labour (3 times), the Order of the Red Star, two Stalin Prizes (1941 and 1950), a Lenin Prize (1957), and numerous medals, scrolls of honour and other decorations.

Announcements

Appointments

Dr. R. G. Priest, St. George's Hospital Medical School, has been appointed to the Chair of Psychiatry, at St. Mary's Hospital Medical School.

Dr. R. R. Trussell, Department of Health and Social Security, has been appointed to the Chair of Obstetrics and Gynaecology at St George's Hospital Medical School.

Miscellaneous

David William Greene, Dublin Institute of Advanced Studies, has been elected President of the Royal Irish Academy, as from March 1973.

Corrigendum

IN the article What Cooks with Solar Neutrinos? by William A. Fowler (*Nature*, 238, 24; 1972), the first part of the second paragraph under "Experimental Physics" should read: "In some rudimentary calculations I have assigned optimum properties to this proposed resonance. The resonant energy, $E_r = 20$ keV, and the width, $\Gamma_r = 10$ eV, were chosen to match the most effective energy and to be less than the effective interval (10 keV) in energy for ${}^3\text{He} + {}^3\text{He}$ in the Sun, respectively. The narrow width chosen also guarantees that the resonance is not detectable at the lowest energies of measurement on ${}^3\text{He}({}^3\text{He}, 2p){}^4\text{He}$. The spin and parity were taken to be O^+ so that an $l=0$ interaction was possible and the dimensionless reduced width in the incoming channel was taken to be unity, yielding $\Gamma({}^3\text{He} + {}^3\text{He}) \approx 10^{-6}$ eV at resonance. The reaction rate from the resonance is $\sim 10^4$ times that extrapolated without resonance from the low energy measurements⁷. This result is approximately independent of the choice of Γ_r as long as it is small compared to 10 keV." I am grateful to Dr M. R. Dwarakanath for pointing out the necessity for this correction.

Reports and Publications

not included in the Monthly Books Supplement
Great Britain and Ireland

Forestry Commission. Guide Map to Your Forests. (London: Publications Officer, Forestry Commission, 1973.) 30p. [291]

Agricultural Research Council. Poultry Research Centre—Summary of Research Reports published in the year ending 31 March, 1971. Pp. viii+56. 35p net. Poultry Research Centre—Summary of Research Reports published in the year ending 31 March, 1972. Pp. xiii+82. 50p net. (London: Agricultural Research Council, 1972. Obtainable from HMSO.) [291]

Universities Research Reactor. Annual Report, 1971/1972. Pp. v+37. (Risley, Warrington: Universities Research Reactor, 1973.) [291]

Scholarships Guide for Commonwealth Postgraduate Students, 1973/1975. Pp. 252. (London: The Association of Commonwealth Universities, 1972.) 65p. [301]

The BBC in the Eighties: The Relationship Between Broadcasting Policy, Programme Needs and Technological Potential. By Charles Curran. (Speech given to the Institution of Electrical Engineers, 14 November, 1972.) Pp. 22. (London: BBC, 1973.) [301]

The Proper Priorities of Science and Technology. By Professor Dennis Gabor. (The Eighteenth Fawley Foundation Lecture.) Pp. 16. (Southampton: The University, 1972.) 20p. [311]

Bulletin of the British Museum (Natural History). Geology. Vol. 21, No. 2: Postcanine Occlusion in Cynodonts and Tritylodontids. By A. W. Crompton. Pp. 27+1+7 plates. £3.40. Zoology. Vol. 24, No. 1: The Genus *Euaugaptilis* (Crustacea, Copepoda). New Descriptions and a Review of the Genus in Relation to *Augaptilus*, *Haloptilus* and *Pseud-augaptilus*. By J. B. L. Matthews. Pp. 1-71. £2.85. (London: British Museum (Natural History), 1972.) [311]

National Institute of Industrial Psychology. Annual Report and Statement of Accounts for the year ended 30 September 1972. Pp. 39. (London: National Institute of Industrial Psychology, 14 Welbeck Street, W1, 1973.) [311]

Other Countries

Smithsonian Contributions to Zoology. No. 134: Additions to Revisions of the Blennioid Fish Genera *Ecsenius* and *Entomacrodus*, with Descriptions of Three New Species of *Ecsenius*. By Victor G. Springer. Pp. iii+13. (Washington, DC: Smithsonian Institution Press, 1972. For sale by US Government Printing Office.) 30 cents. [181]

Sudan. Agricultural Research Corporation. Technical Bulletin No. 3 (New Series): Leaf Curl of Tomato. By A. M. Yassin and H. S. Abu Salih. Pp. 24. (Hudaida: Botany and Plant Pathology Sections, The Research Station, Agricultural Research Corporation, 1972.) [181]

US Department of the Interior: Geological Survey. Bulletin 1314-F: Trace-Element Contents of Some Plutonic Rocks of the Sierra Nevada Batholith. By F. C. W. Dodge. Pp. iii+13. 55 cents. Water-Supply Paper 1939-D: Chemical Quality of the Water in the Tucson Basin, Arizona. By R. L. Laney. Pp. iv+46+5 plates. Water-Supply Paper 2016: Quality of Surface Waters of the United States, 1967. Parts 12-16: North Pacific Slope Basins, Alaska, and Hawaii and other Pacific Areas. Pp. xvi+431. \$2. Professional Paper 562-J: Summary of Alluvial-Channel Data from Rio Grande Conveyance Channel, New Mexico, 1865-69. By J. K. Culbertson, C. H. Scott and J. P. Bennett. Pp. iii+49. 70 cents. Professional Paper 691: Stratigraphy and Origin of the Triassic Moenkopi Formation and Related Strata in the Colorado Plateau Region. By J. H. Stewart, F. G. Poole and R. F. Wilson. Pp. v+195+5 plates. Professional Paper 741: The Quinary Reciprocal Salt System Na, K, Mg, Ca/Cl, SO₄—a Review of the Literature with New Data. By J. J. Rowe, G. W. Morey and C. S. Zen. Pp. vi+37. 50 cents. Professional Paper 745: Geology and Uranium Deposits, Shirley Basin Area, Wyoming. By E. N. Harsham. Pp. vii+82. Professional Paper 760: Paleomagnetism of some Lake Superior Keweenaw Rocks. By Kenneth G. Books. Pp. iv+42. 50 cents. (Washington, DC: Government Printing Office, 1972.) [181]

HOW TO BUY NATURE

The cost of one year's subscription to NATURE is:

	UK & elsewhere	US & Canada
Nature (Friday)	£16	\$48
Nature & Nature Physical Science or Nature New Biology	£24	\$83
Nature New Biology or Nature Physical Science	£10	\$35
All three editions	£29.50	\$108

(Charges for delivery by air mail on application). Subscribers in North America may be able to claim a tax rebate against their NATURE subscription.

Editorial, Advertising and Publishing Offices of NATURE

MACMILLAN JOURNALS LIMITED
4 LITTLE ESSEX STREET, LONDON WC2R 3LF
Telephone Number: 01-836 6633. Telegrams: Phusis London WC2R 3LF
Telex 262024

MACMILLAN JOURNALS LIMITED
711 NATIONAL PRESS BUILDING
WASHINGTON DC 20004
Telephone Number: 202-737 2355. Telex 64280

International Advertisement Manager
PETER R. KAVANAGH
MACMILLAN JOURNALS LIMITED
4 LITTLE ESSEX STREET, LONDON WC2R 3LF
Telephone Numbers: UK 01-836 6633. USA 202-737 2355

Subscription Department
MACMILLAN JOURNALS LIMITED
BRUNEL ROAD, BASINGSTOKE, HANTS RG21 2XS
Telephone Number: Basingstoke 29242

Classified advertisements
T. G. SCOTT & SON, LIMITED
1 CLEMENT'S INN, LONDON WC2A 2ED
Telephone Number: 01-242 6264/01-405 4743
Telegrams: Textualist London WC2A 2ED

Registered as a newspaper at the Post Office
Copyright © Macmillan Journals Limited, April 6 1973